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AN EXPLORATORY INVESTIGATION OF LANGUAGE BY MEANS OF OSCILLOGRAPHIC AND REACTION TIME TECHNIQUES *

BY N. H. PRONKO

Indiana University

AN OUTLINE OF THE PROBLEM

Current language theories may be placed into one of two categories depending on their assertion about the directness or indirectness of the behavior involved. These theories may be designated as Adjustmental or Direct, and Symbolic.

A. Adjustmental or direct.—Here the speaker reacts to the hearer and the things spoken about simultaneously as in telling someone about something. On the hearer's side the action is likewise direct and bistimulational since he is interacting with the speaker's action and the thing talked about at the same time. In plain words, he is being told about something by someone.

B. Symbolic or indirect.—Words issuing from the speaker are considered as substitute stimuli for evoking ideas or emotions in the hearer or as substitute stimuli for things.

These theories set the problem for the present experiment and suggest the observation and analysis of two clear-cut linguistic situations to be designated and differentiated in the following manner:

A. Direct or non-substitutive language situation.—The *S* is presented with a series of slides exposed on a screen in front of him; these represent common objects or events. He is instructed to answer (i.e., to refer the *E*) to each slide when presented as quickly as possible by stating what is depicted on it.

B. Substitutive or indirect language situation.—The *S* (following the series in *A*) is shown the same slides as in *A* but with certain nonsense syllables associated with each, which he is required to connect in a single practice series during which both are exposed simultaneously. After one such practice series the nonsense syllables alone are exposed and the *S* is instructed to react as quickly as possible with the name of the corresponding object. In short, the *S* must here perform an indirect action with the nonsense syllable serving as a substitute stimulus for the associated object. This substitutive action is the essential difference between Situations *A* and *B*.

* This report is part of a dissertation for the degree of Doctor of Philosophy in Indiana University. The writer wishes to express his thanks to Dr. J. R. Kantor for his direction and assistance.

Beyond this point, the *S*'s reactions to successive practice series of slide presentations are observed, recorded and analyzed in order to determine if the substitutive reaction crosses over into a direct act.

The following criteria may serve to differentiate substitutive from direct language action.

1. *Reaction time.*—After continued practice, the initially long reaction times of the early substitutive (*B*) situation will be compared with the RTs obtained in the non-substitutive (*A*) situation. A further check is had by comparing with a final series of the direct or *A* reactions.
2. *Action potentials.*—Davis (**7, 8, 9, 10, 11**) has shown that presence or absence of complex psychological action may be indicated by muscle action potentials. The same technique is applied here with the intention of distinguishing the behaviors in *A* and *B*, the underlying assumption being that the behavioral differences (smoothness of direct as contrasted with indirect action) may show slight differences in muscle action as detected by the oscillograph.
3. Any other behavioral items to be derived from direct observation of the two behavior situations.

The Experiment

1. *Apparatus*

The apparatus employed in the present experiment consisted of two rather distinct systems: (1) a cathode ray oscillograph, amplifier, and camera for recording muscle potentials, and (2) a stimulus-presentation and reaction time device.

Oscillographic recording.—The cathode ray oscillograph in use here has been described by Davis (**7, 8**). Shielded leads from a booth enclosed in sheet metal and copper screen were led into an adjoining recording room where they were connected to an amplifier, the voltage output of which was fed into a cathode ray oscillograph having a five-inch screen. Gain of this amplifier was adjustable in accordance with the variable size of the muscle potentials from the *S*'s, and record was kept of settings so that readings could be reduced to common units.

A second oscilloscope was connected in parallel with a stimulus light and Stoelting chronoscope (to be described below), so that simultaneously with depression of the stimulus key the alternating current in the light circuit was introduced into it. The result was a deflection pattern consisting of a 60-cycle wave lasting for the duration of the stimulus. This was reflected via mirror into the same camera that recorded the action potentials. In addition, a tube filled with argon gas and placed within the field of the camera flashed 60 times per sec., furnishing a convenient time record. In this manner, action potentials, onset and cessation of stimulus, and a time-line were simultaneously recorded on the same film. Photographing was done by means of a moving film camera loaded with 35 mm. Eastman recording paper. The camera was controlled by telegraph key placed at *E*'s table.

Stimulus presentation.—Stimulus presentation was effected through an exposure apparatus, a black sheetmetal box illuminated from within by a 60-watt frosted light bulb. The front of the exposure box had a slotted opening in which a frosted-glass screen was inserted. Between this screen and front of box was a channeled guide permitting insertion of the experimental slides (Fig. 1) which were drawn in outline form with india ink on window glass, size $6'' \times 4\frac{1}{4}''$, and were protected by a thin coat of clear varnish. The slides could be presented so that the illustrated object and corresponding nonsense syllable, or both, would be exposed.

After a flap of sheet metal of the appropriate size had been cut in the wall of the booth, the front of the stimulus apparatus was placed directly against the outside screening, which was left intact. The slides were now in plain view of the *S*. When the flap was next fitted over the top of the exposure box and all outside light excluded from the screen, the slides could be seen only when the light was flashed on inside the box. This was accomplished by depressing a telegraph key that simultaneously illuminated the screen, started a 60-cycle wave deflection in the second oscilloscope (previously referred to), and started a chronoscope.

Reaction-time recording.—The *S*'s key consisted of a microphone attached by a ring-stand to the broad writing arm of a chair placed in the booth facing the exposure apparatus. The microphone, which could be adjusted horizontally and vertically for the individual *S*, was fed

through an audio-amplifier, the output of which was adjustable to such a level that a cold-cathode gastriode tube in circuit with it just failed to discharge. However, an impulse from the microphone furnished a boost in voltage sufficient to overcome the resistance of the gas filled tube, the discharge of which actuated a relay circuit control switch. The operation of the relay switch caused a break in the circuit established by *E* in closing the stimulus key; this was recorded on the film as a discontinuance of stimulus. At the same time, the chronoscope was stopped (giving

FIG. 1. Illustrating slides used in the present experiment. These could be presented so that the pictured object and its corresponding nonsense word might be exposed simultaneously or separately. Nos. 1–3 show common objects; 4–6 show action.

a measure of the *S*'s voice reaction time) and the stimulus light in the exposure box was extinguished. The advantages of such a set-up have been described at greater length by both Hamburger (12) and Bucklew (5).

2. *Procedure*

With the apparatus described above functioning in the manner indicated, the first operation consisted in applying electrodes to each *S*. The active lead, providing contact by means of a nickeled disk 5 mm. in diameter, was attached to one side of an earphone head piece and was

held comfortably in place over the *S*'s right masseter muscle. This particular location was selected only after exploratory work indicated fairly large potentials. The inactive lead was taken from the neck with a wet felt pad.

The direct series.—While the *S* was relaxing and adjusting generally to the experimental situation he was handed a card with the following instructions on it:

> In this part of the experiment slides depicting certain objects and happenings will be presented to you. As soon as they are flashed on, you must say as quickly as possible what the object or event is. In ordinary conversation you would probably turn to me and tell me what you saw; but, since this is an experimental situation, it is necessary to formalize your report by asking you to address your remarks through the microphone. In every instance, you must speak up as quickly as possible since the time you require to respond will be measured by a chronoscope and will be recorded. Pronounce loudly and distinctly.
>
> There will be two groups of slides—three with common objects and three illustrating people in action. You may refer to the former with a single word; to the latter, with several words as, for example, 'Plane diving' in the case of an airplane going into a nose-dive.
>
> Once we have started the experiment ask no questions or make any comments until we have finished.
>
> This experiment is not a test of your intelligence or any other ability. It is simply a study of the language behavior of individuals.
>
> Be as quiet as possible and do not move after the ready signal has been given.

After the *S* had had sufficient time to study the instructions, he was asked if the directions were clear. Then, regardless of the answer, *E* showed him the chronoscope and demonstrated how *E*'s key started the clock and flashed the slide, and how *S*'s verbal report through the microphone stopped the clock and put out the stimulus light. The door of the booth was then closed and stimulus presentation, begun.

The slides, exposed after a 2-sec. interval following a 'ready' signal, were presented about 15 sec. apart. After each series, the slides were well shuffled and shown in chance order so as to preclude anticipation on the part of the *S*. The first series of six slides was considered a *practice* series and the times were consequently discarded. The third series yielded both muscle potentials and reaction times, but the only change in procedure entailed closing the camera key simultaneously with the ready signal.

The practice series.—It should be noted that the first 18 trials (three series) consisted of referring directly to the pictures themselves; for the following series consisted of observing the nonsense syllables (not shown previously) which were to be associated with the corresponding picture. This was again explained to the *S* in directions presented to him on a card reading as follows:

> The purpose of this part of the experiment is to note how quickly you can learn to associate certain nonsense syllables with the objects and happenings just presented to you on the slides.
>
> As each slide is shown you, look first at the nonsense syllable above the picture, then at the picture below it. When you have done that *respond as quickly as possible in the same way you did previously.*
>
> Make it your whole task to connect the two because in subsequent series you will be presented with the nonsense syllables alone and will be required to respond to them just as you have been responding to the corresponding objects.
>
> Don't move after you hear the Ready signal.

The booth door was now closed and the full slides shown for a period of one sec., during which time action potentials were again recorded as a check on the muscle action to be recorded in the next part of the experiment.

The indirect series.—After a single presentation of the practice or learning series, the *S* was initiated into the third part of the experimental procedure with the following directions presented as before:

> Some nonsense syllables will now be flashed on the slides before you. You must answer as quickly as possible with the same word or words that you used when their corresponding pictures were shown you before. Answer distinctly and as fast as possible since your

reaction time will be noted and recorded. You are not expected to know the complete list the first time. Just do the best you can. In case you don't know, just answer, "I don't know."

Don't move after the Ready signal is given.

The nonsense syllables were presented in the same manner as the pictures themselves had been, both as to fore-period and interval between trials, which procedure was maintained in subsequent series of this indirect sort. After the first indirect series another practice series was given, during which both syllable and picture were exposed. For certain *S*s two exposures were sufficient; for others, practice series were alternated with the indirect or test series until a criterion was reached on one errorless response to a series. At this stage, the *S* was instructed that he needed no more learning trials and that his task from now on was to make the screen light go off as fast as possible by reacting as quickly as he could. This was found to furnish the modicum of motivation needed to forestall boredom and monotony induced by the repetition of a rather simple task.

When the reaction time was reduced to within 0.1 sec. of the RT to the original pictured objects, the experiment was terminated. This criterion was decided upon after observing as much variation as this between any two succeeding trials.

In case *S* had difficulty in associating names with objects, the experiment was terminated after the twentieth trial, so that no *S* had over 20 trials during the session.

Continued practice.—Since it was necessary for the hypothesis tested that the indirect response approach the reaction time to the direct object after continued practice, 13 *S*s selected at random were asked to come to the laboratory a week following the first session. These were put through the same procedure as before and were presented the nonsense syllables *without any practice series*. In every case, the *S* was told that he would be given 20 series of the nonsense syllables alone and that he was to repond as quickly as possible each time. The twentieth trial was photographed and a final series was given with the objects presented alone, during which action potentials were again recorded and compared.

A control check.—One possible criticism against the procedure as outlined above may be directed against the materials employed. Since the two classes of stimulus objects (nonsense syllables and pictures) are hardly equivalent at the start, it would be well to know their relative reaction time values. In order to make this check, it was decided to run four more *S*s but in a reverse direction; i.e., these *S*s would be presented the nonsense syllable first, then the pictured objects, with instructions changed so as to require the pronunciation of the nonsense syllable name to presentation of the picture alone. In this manner, time differences inherent in the discrepant materials themselves might be determined, and a more valid evaluation of the general reaction times arrived at.

Such a procedure was accordingly carried out with the changes indicated. In the direct series the following instructions were substituted for the original ones:

In this part of the experiment, certain nonsense syllables will be flashed on a screen in front of you. You must answer each time as quickly as possible since your reaction time will be noted and recorded.

Once we have started the experiment, ask no questions or make any comments until we have finished.

This experiment is not a test of your intelligence or any other ability. It is simply a study of the language behavior of individuals.

Do not move after the 'ready' signal has been given.

In the practice series the *S* was instructed as follows:

The purpose of this part of the experiment is to note how quickly you can learn to associate certain objects with the nonsense syllables just shown you.

As each slide is presented to you, look first at the picture of the object, then at the nonsense syllable and *respond with the nonsense syllable as before.*

Make it your whole task to connect the two because in subsequent series you will be presented with the picture alone and will be asked to respond to it as you have been to the nonsense syllable.

Don't move after you hear the 'ready' signal. Be as relaxed as possible.

The differences in the procedure used with the two groups may be shown schematically in the following plan.

Experimental Group	Control Group
1. Presented with objects	1. Presented with names
2. Presented with corresponding names of objects	2. Presented with pictures corresponding to names
3. Practice given in responding to nonsense name (presented alone) with the response that had been given to the corresponding object	3. Practice given in responding with nonsense name to objects presented alone
4. Comparison of RT and action potentials of responses originally given to objects with those now made to their nonsense names	4. Comparison of RT and action potentials of responses made originally to nonsense names with responses now made to the corresponding objects

3. *Treatment of Results*

Reaction time.—Reaction times were noted and recorded for each reaction of each *S*. These were then averaged for each series of six slides, this figure entering into further calculations that permitted comparison of group performance under different experimental situations.

Action potentials.—Since a myogram is a continuous record, one must necessarily divide it into arbitrary portions in order to derive any sort of quantitative relationships with other factors in the experimental design. A series of preliminary measurements of records suggested the advisability of measuring the amount of deflection at points separated by 0.1 sec. intervals. More particular details of the analysis of action potential data are given later.

Results and Discussion

1. *Primary Reaction Time Findings*

Table I shows the average RTs for all 50 *S*s. The direct [1] series give a mean RT of 555 ms.; this is to be compared with an average of 1343 ms. for the test [2] series. It will be recalled that this was the run *following* the series in which the *S* had been shown both object and associated name but which shows response time to the name presented alone. That the difference is a statistically significant one is indicated by a critical ratio of 10.65.

After continued reaction for a maximum of 20 trials, all *S*s furthermore show an average RT of 634 ms., which has been reduced downward to approximately the original response time, but which the critical ratio of 5.84 shows to be still significantly different. This might indicate that the responses which were indirectly performed via the nonsense name substitute stimulus are still indirect in the final test series for all *S*s. But another interpretation is possible when it is pointed out that for affective and other reasons, 17 of the *S*s failed in accomplishing the task.

[1] Direct series refers to the original response to the first presentation of the object or action illustrated on the slide.

[2] The final test series were those during which the *S* was learning to respond to the nonsense name alone, the rest of the slide being covered up, which practice was continued until the *S*'s RT met the criterion or until he had been given a maximum of 20 trials.

TABLE I

REACTION TIME DATA FOR ALL (50) SUBJECTS COMBINING AVERAGES OF THOSE MEETING THE CRITERION AND THOSE FAILING TO MEET IT (UNITS IN MILLISECONDS)

Series	Mean RT	SD	SD_M
Direct	555	78.5	11.2
Test	1343	529.0	76.5
Final Test	634	85.6	12.2
Difference	(Direct-Test)		788.2
$SD_{difference}$	(Direct-Test)*		74.0
$\frac{Difference}{SD_{difference}}$	(Direct-Test)		10.65
Difference	(Direct-Final Test)		79.0
$SD_{difference}$	(Direct-Final Test)		13.5
$\frac{Difference}{SD_{difference}}$	(Direct-Final Test)		5.84
Difference	(Test-Final Test)		709.2
$SD_{difference}$	(Test-Final Test)		75.8
$\frac{Difference}{SD_{difference}}$	(Test-Final Test)		9.35

*The formula for the standard error of a difference between the means of related variables given by Lindquist (**21**, p. 125) was employed here as well as in subsequent tables.

When the data are separated [3] for the group failing to meet the criterion from that which did meet it in 20 trials or less, the results are strikingly different, as indicated in Tables II and III. It will be seen in Table II that the direct response, with an average RT of 575.9 ms. for the 33 *Ss* of this group (designated as the Criterion group), compares favorably with final test RT of 602.5 ms. for the same group, as indicated by the significance ratio of 2.68 derived from these measures. The case for the similarities of the final test and the direct series with respect to reaction time is further substantiated by the extremely divergent results in a comparison of averages for the final test and the test series. The mean RT for the test series is 1401.9 ms. compared with 602.5 for the final test as pointed out above, the critical ratio in this case being 7.29.

[3] To adhere stubbornly to preconceived notions about the analytic treatment of data without recognition of the variables and outcomes of the experimental situation would not be profitable. Precepts for the separate treatment of results indicated above abound in the experimental literature, as may be seen in the work of Hilgard (**14**) and others. Further justification for the separate handling of data exists in the additional light such variations may throw upon the very problem under observation.

TABLE II

REACTION TIMES IN MILLISECONDS UNDER VARIOUS EXPERIMENTAL CONDITIONS FOR 33 SUBJECTS MEETING CRITERION

Series	Mean RT	SD	SD_M
Direct	575.9	76.4	13.7
Test	1401.9	615.8	110.6
Final Test	602.5	65.9	11.8
Difference	(Direct-Test)		825.9
$SD_{difference}$	(Direct-Test)		113.5
$\frac{Difference}{SD_{difference}}$	(Direct-Test)		72.8
Difference	(Direct-Final Test)		26.6
$SD_{difference}$	(Direct-Final Test)		9.9
$\frac{Difference}{SD_{difference}}$	(Direct-Final Test)		26.8
Difference	(Test-Final Test)		799.4
$SD_{difference}$	(Test-Final Test)		109.7
$\frac{Difference}{SD_{difference}}$	(Test-Final Test)		73.9

TABLE III

REACTION TIMES IN MILLISECONDS UNDER VARIOUS EXPERIMENTAL CONDITIONS FOR 17 SUBJECTS FAILING TO MEET CRITERION

Series	Mean RT	SD	SD_M
Direct	517.8	67.6	15.9
Test	1238.0	293.2	69.1
Final Test	690.0	87.8	20.7
Difference	(Direct-Test)		721.1
$SD_{difference}$	(Direct-Test)		66.1
$\frac{Difference}{SD_{difference}}$	(Direct-Test)		109.1
Difference	(Direct-Final Test)		172.2
$SD_{difference}$	(Direct-Final Test)		18.1
$\frac{Difference}{SD_{difference}}$	(Direct-Final Test)		95.0
Difference	(Test-Final Test)		548.9
$SD_{difference}$	(Test-Final Test)		66.4
$\frac{Difference}{SD_{difference}}$	(Test-Final Test)		82.7

2. *Interpretation of RT Data*

In line with our hypothesis, we may interpret the divergences between direct and test and test and final test and the correspondence between the direct and final test as follows. In the direct response, the *S* is performing a single direct linguistic reaction as he normally does when he refers someone to an object (the location of something, presence of a person, et cetera). This is referor behavior—a point which cannot be emphasized too strongly, since the symbolic theory of language, which is typically concerned with the 'symbol' or 'thing' (i.e., the vocal action of the speaker), deals with these 'products' from the viewpoint of the hearer, but such a theory cannot give a symbolic explanation of the *speaker's* action such as occurs in the present (direct) situation where the *S* tells the *E* what he (the *S*) sees before him. Apparently, the symbolic theory is not equipped for handling such an event (except for an implicit leaning upon an expressionistic construction). The only explanation one might give in operating from a symbolic base is the extremely attenuated one that the object which the *S* sees is a 'symbol' for him that 'tells' *him* what to say, an argument which would make a 'symbol' out of every stimulus object in all sorts of behavior segments and which would be an obvious *reductio ad absurdum*.

From the viewpoint of the present investigation, to introduce a symbol into the referor situation as described above would be to impose a highly strained explanation upon a fact explainable less fancifully and more objectively. In interbehavioral terms, such a description would conceive of a speaker's situation as a response performed in as direct a manner as any type of manipulative reaction, the only difference being its bistimulational nature. There is no demand for a symbol here any more than in a perceptual, affective or handling response.

Leaving this point for the moment, it will be recalled that the direct series was followed by a practice series during which the *S* was instructed to connect up a specific nonsense name with a particular object or event depicted on a slide. Only one such practice series was given; following it, the object was hidden from view and only the correlated name was exposed, the *S* now being required to respond with the same response he had previously made to the associated object; this, then, is obviously a symbolic situation, in which the *E* has set up for the *S* a 'symbol' (i.e., the nonsense name) and a 'designatum' (object on slide for which the symbol stands or to which it refers).[4] Note that there is a strict one-to-one relationship, as

[4] If the 'symbolinguist' still persists in a symbolic explanation for the referor behavior, in the direct series, then he must consequently call this a double symbol situation since an intervening symbol has been added in the form of a nonsense name.

demanded by Ogden and Richard's (20, Chap. V) canons of symbolism; but the important point is that this is *referee* behavior, in which the *S is referred* to an absent object through its symbol.[5] That there is an intervening associative action is indicated by the greatly lengthened RTs of the *S*s (about $2\frac{1}{2}$ times) in the test series as compared with their original (direct) referor RT.[6]

For clearness of exposition it should be noted again that, following the test series, the *S* was given continued presentation of nonsense name alone (without its 'designatum') until his response had come within 0.1 sec. of his original response or until he had been given a total of 20 series, at which time the practice series was discontinued. It was found that with repetition the situation changed from a 'referee' to a 'referor' one, as indicated by the RTs of these *S*s. The *S*s were evidently performing the same behavior morphologically as they had made to the original objects and scenes on the slides at the beginning of the experiment. Practice of the specific response had apparently changed a symbolic response into a linguistic (referential) one.

There are occurrences from every day life that are analogous to the situation described above. For example, a student learning a foreign language must translate his foreign words into English equivalents before getting the sense of a passage; but, with practice, this indirect behavior drops out and the fluent reader may be referred directly to the object talked about without going through a symbolic process.

The 17 *S*s whose RTs are shown in Table III apparently failed to make the transition to a direct response, as indicated by the critical ratios derived from a comparison of the direct and final test (C.R. = 9.50) and the test and final test series (C.R. = 8.27). In other words, the responses in their case remain *differentiated* instead of approximating each other; and, although the test series is much longer than the original response of the direct series, it stays that way and does not approach the direct by the end of the experiment.

3. *Support from Experimental Literature*

Several experiments have been reported in the literature, which, although designed to study problems only distantly related to the present investigation, show results which substantiate the argument developed up to this point.

In an experiment which set out to study the efficacy of associative aids in the learning of two unrelated words by use of the RT technique, Reed (23) took lists of words like *sauce-balloon*, *mistake-clean*, *elephant-steeple*, etc., read them once to the *S*, and then used the prompting method

[5] This is the same as Bloomfield's (2, p. 30) 'displaced speech.'

[6] This is not an experimental artifact due to extremely long RTs as the result of the *S*'s failure to reply, inasmuch as such occurrences were omitted from a calculation of the means.

requiring the *S* to respond with the second word of each pair as soon as possible after *E* gave the first word. Each *S* was asked to report what he had 'thought' between stimulus and response.

Reed's results showed that intermediate associative aids were used by the *S*s. For example, one *S* first connected *stencil* with *kimono* (in the pair, *kimono-stencil*) by thinking of a design stencilled on a kimono, when the stimulus word, *kimono*, was presented. Reed believes that the use of such devices between two unrelated terms makes for an easy transition from one term to another unrelated term, but that they tend to drop out with practice. In his own words, Reed (**23**, pp. 261–262) concludes that

> . . . associative aids disappear with practice and that their disappearance shortens the RT. My records show that a pair that was accompanied by an associative aid on the sixth day always had a longer reaction time than one that was not so accompanied. *This is what we should expect, for, other things being equal, the more middle terms between a stimulus and a response, the longer the RT would reasonably be.*[7] From this principle as well as from the facts of Table IX, it is safe to assume that the disappearance of these middle terms is another factor conditioning the rate of improvement from day to day.

In a follow-up paper, Reed (**24**) stated that when the *S* utilizes some middle term in early practice, he would call it 'thinking' (implicit action?) but that:

> . . . with continued repetition the response becomes gradually unconscious and is made without attention. That is, it becomes a type of reflex action [*sic*]. Where this point is reached, there is not thinking in the psychological sense of the word, it is simply habit. (Pp. 379–380.)

The present writer would similarly conclude, although not in Reed's terminology, that use of the symbol (*as a symbol* for the absent object) tends to drop out and becomes a stimulus in its own right, eliciting a response in as direct a fashion as that elicited by the original objects and actions depicted on the slides. The directness of the action may be compared to the directness (although not the speed, of course) of an avoidance reaction.

Further testimony for the interpretation developed here is afforded by an experiment of Leuba and Hyde (**20**). These workers assigned their *S*s the task of putting English prose into German script and vice versa. The German 'language' was not involved, since the writers may have devised shapes of their own design to serve as symbols for the English letters. But our interest is in the evaluation of the response that the *S*s practiced. Whereas in early practice each letter was reacted to individually, later, groups of letters, syllables, or even words were responded to without an intermediate translation process.

> At this culminating point . . . the writing follows immediately—automatically—upon the presentation of the German letters, itself preceded by nothing more than the perception of the English letters. (P. 357.)

Book's (**3**) study of typewriting by 'touch' showed similar dropping out of intervening responses, such as pronouncing the letter seen on the copy and locating it implicitly on the keyboard, until the sight of the copy led *directly* to striking the proper keys. The well-known Bryan and Harter (**4**) study shows a similar evolution of a direct response from a 'symbolic' one, and an abundance of illustrations is available in the field of conditioning. In recent times, the work of Hilgard (**13**), Warner (**26**), Zener (**28**), Upton (**25**), and Kellogg and Wolf (**19**) makes the classical interpretation of the conditioned stimulus as a 'symbol' a questionable one and seems to point to the directness of responses to both stimuli (CS and US). However, to carry the argument further would be to belabor a point.

4. *Results of a Check Experiment*

It was pointed out in the procedure that the nonsense name and correlated object may not have been equivalent perceptually, thus containing inherent differences in reaction time that might be ascribed to other factors. It may well be that such differences would

[7] Italics are the present author's.

prevent the *Ss* from ever attaining as low an RT to the nonsense names as to the objects, a condition that would complicate any test of the experimental hypothesis. As a check against such an uncontrolled factor, it was decided to run four *Ss* in the opposite direction. For these *Ss*, the object or action on the slide served as a 'symbol' or substitute stimulus to which they were to respond with its correlated name. Table IV shows results which seem to confirm

TABLE IV

MEAN RT IN MILLISECONDS FOR A CONTROL GROUP OF FOUR SUBJECTS PRESENTED WITH PICTURE FIRST AND THEN NONSENSE SYLLABLE

Series	Mean RT in ms.
Direct	448
Test	1120
Final Test	507

suspicions of the advantages of the illustrations of objects and persons; for it will be noted that the mean initial RT for these responses to object is 448 ms.—a figure lower than the initial RTs shown in Tables I–III (using the opposite procedure) with mean RTs of 555 ms. to the nonsense name when data from criterion and non-criterion groups are combined. Following continued practice, these *Ss* reduced their RTs for the response to the 'object' stimulus to 507 ms. as compared with 602.5 ms. for our better *Ss* (the criterion group).

While the sampling is too small to permit accurate tests of statistical significance, the results seem to indicate that although our *Ss* met the criterion, they were working against disadvantages incurred by the selection of stimulus materials. Further proof appears when we compare 13 *Ss* from the original group, selected at random, with these four *Ss* in a follow-up session given them one week following the first meeting. The data presented in Tables V and VI show a

TABLE V

REACTION TIMES (IN MS.) FOR 13 *S*'s GIVEN CONTINUED PRACTICE FOR 20 TRIALS BEYOND THE ORIGINAL SESSION AND SPACED ONE WEEK FOLLOWING IT

Trial Number	RT in ms.
1	990.3
2	649.5
5	599.1
10	584.3
15	569.4
20	575.1
21 (directly to object)	525.3

persistent advantage for the pictured objects and actions of persons. Fig. 2 drawn from these data again illustrates the superiority of the pictured objects over the names, which point may be used to strengthen the significance of the results that we did obtain. This would also point to the necessity for using equivalent stimuli in future

TABLE VI

REACTION TIMES (IN MS.) FOR 4 *Ss* GIVEN CONTINUED PRACTICE OF 20 TRIALS EACH ONE WEEK FOLLOWING ORIGINAL SESSION, AS FOR *Ss* IN TABLE V, EXCEPT THAT THESE *Ss* RESPONDED TO PAIRED PICTURE INSTEAD OF NONSENSE SYLLABLE (i.e. IN REVERSED ORDER)

Trial Number	RT in ms.
1	1050.0
2	707.3
5	533.6
10	572.8
15	532.9
20	439.1
21 (directly to nonsense syllable)	595.1

investigations along such lines. It is interesting to note that Cattell (6) found a similar advantage in favor of pictures when he made his well-known study of reaction time.

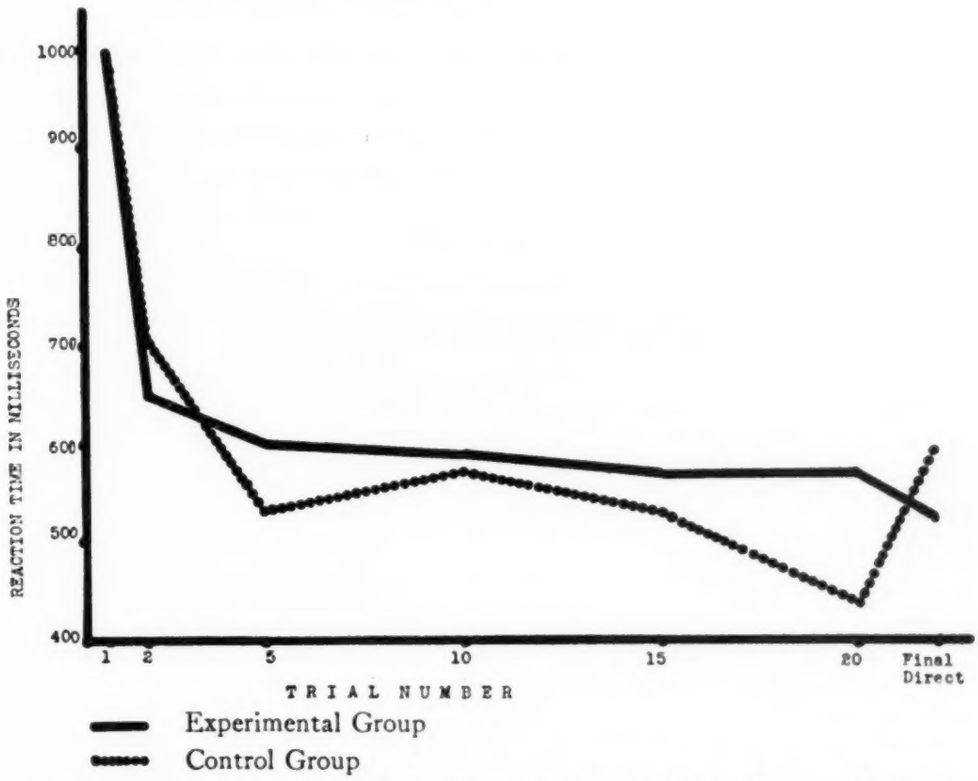

FIG. 2. A comparison of the effects of continued practice in the experimental and the control groups. Curves drawn from data presented in Tables V and VI showing sudden drop in RT when continued practice was given for 20 trials one week following the first session. Experimental Group—13 *Ss* that had been presented nonsense syllable, then object, continuing practice to syllable presented alone. Control Group—4 *Ss* presented with object first, then nonsense syllable, with continued practice to object presented alone.

One final point should be made with regard to the two curves in Fig. 2. Both show a slow reaction in the first trial, no doubt indicating a return to a 'symbolic' reaction due to the week's interval without practice; but note the quick drop on the very next trial, with little subsequent change during the rest of the session, except for the expected variability of the unusually small control group.

5. *Further Evidence for a Bistimulational Response*

Evidence from individual observations on two *S*s, one from the criterion group, the other from the non-criterion group, is pertinent here. The results show that after only nine trials a criterion *S*, Sch, responded as quickly to the nonsense syllables alone as he had originally to the correlated pictures. The other *S*, Ham, failed to do so after 20 trials; in fact, he responded even slower at the end than at the beginning to the *same* stimulus (depicted object). Greater variability was also noted in the second case. A rather common reason for such irregular performance was found to be boredom or other affective complications. Since the *S*s were not informed of the purpose of the experiment, their task necessarily seemed artificial and monotonous.

Indirectness of response and long RT.—One observation made repeatedly throughout the experiment when the 'designatum' was hidden from the *S*'s view was the greatly lengthened reaction time shown in the case of responses that were unmistakably 'symbolic' or substitutive cases. Several of these, presented in Table VII, show

TABLE VII

A Comparison of RTs of the Experimental Group When either No Response or When Two Verbal Responses were Made to the Presentation of the Nonsense Syllable alone

Slide	First Response	Second Response	RT in ms.
TEF-HAT	Uh– – –	Chair	1000
TEF-HAT	Top, uh– – –*	Hat	960
JOD-CHAIR	Davenport, uh– – –**	Chair	2150
JOD-CHAIR	Hat, uh– – –	Chair	1020
JOD-CHAIR	Bugle, uh– – –	Chair	1610
JOD-CHAIR	Uh– – –	Chair	1570
JOD-CHAIR	No response (D. K.)		2750

* After the experiment *S* explained that his mnemonic device was to "think of Top, then Hat as soon as I saw Tef."

** A similar explanation was made for the 'davenport' response. The 'D' in JOD suggested 'davenport,' which suggested the 'chair' response. Both of these responses are characteristic response of the indirect process of the early stages of learning. Note that RTs are correspondingly long for this multi-segment type of behavior.

unusually long RTs, ranging from 960 to 2750 ms. These were in every instance related to absence of response, hesitancy, or intervening responses; as such they add additional argument for the directness of responses when RTs are very short.

Argument from variability of verbal patterning.—A symbolic relationship bears a strict one-to-one correspondence between the 'symbol' and its 'designatum'; e.g., the numeral, 7, designates the number of a collection of objects, or it might be said to 'symbolize' the number of such things, for another figure cannot rightfully be

substituted for the 7 without disrupting the strict relationship between them. Such a strict correspondence of symbol and symbolized offers a good test of a symbolic situation, and, if found in the present experiment, would tend to destroy the argument for the direct response that has developed to this point. On the other hand, presence of a widely-varying substitution or modification of the verbal responses made originally to the direct object when that object was no longer present, but only its surrogate, would add further corroborative proof against the existence of a symbolic relationship here.

Such unexpected evidence did, in fact, appear throughout the experiment, and was at first not recognized as such, but was merely recorded as a wrong response; therefore, accurate count of all instances is not available. However, once the significance of such responses was appreciated, their occurrence was recorded. These are presented in Table VIII, and show a total of 71 changes in the *S*'s verbal patterning—not to the object or action portrayed on the slide—but to the associated nonsense stimuli (the rest of the slide being here screened from the *S*'s view).

TABLE VIII

SHOWING THE NUMBER OF CHANGES IN THE VERBAL PATTERNING OF *S*'s RESPONSE TO EACH OF THE SLIDES

Slide	Number of Changes					
	TEF * Hat	JOD * Chair	ZAG * Umbrella	RUC ** Girl Skating	WOJ ** Bugler	LEM ** Baby Playing
Total Changes	3	5	1	13	23	26
Grand Totals		9			62	

* Simple slide representing a common object.
** Complex slide representing humans in action.

Table VIII shows, furthermore, that most of the changes, 62 in number, occurred with the presentation of the nonsense names associated with the slides depicting complex scenes of humans in action. Thirteen variations occurred to RUC, accompanying a slide showing a girl skating, while 23 and 26 changes were recorded for WOJ and LEM respectively—names connected with a boy blowing a bugle and a baby playing on the floor.

The striking differences in verbal patterning changes between the complex and simple slides suggest that such variability is a function of the materials used. It seems reasonable to assume that the simple objects set a limiting condition upon *possibilities* for changing the style of a response. One can scarcely call an umbrella

anything but an umbrella; however, even here the *S* who had originally referred to the picture of an umbrella with the word-response, 'umbrella,' changed her response to 'parasol' and back again to 'umbrella' during the period in which *only the correlated name*, ZAG, was being presented to her. Further check on the effect of the complexity of the stimulus object on response features should be made. At any rate such changes as were observed throw serious doubt upon the correctness of a 'symbolic' interpretation of the *S*'s linguistic behavior in the late practice period. On the contrary, they suggest that an object other than the picture (i.e., the nonsense name) now has a linguistic stimulus function that originally inhered in the picture. Besides the changes in verbal patterning as such, the differential frequency of such change correlated with simplicity and complexity features of the stimuli offers further embarrassment to symbolic attempts at explanation of the results obtained in the present experiment.

While the foregoing account detracts from a symbolic construction of the results, it is legitimate to ask what happened to the reaction times in the cases of changes in the stylistic reference-making referred to. If the times should be considerably lengthened, it might be argued that intervening 'symbolic' responses were occurring. On the other hand, if the times are about the same or shorter despite the change in response patterning, it would all the more definitely point to the directness of these responses.

Table IX shows randomly-selected verbal responses of *S*s with their corresponding reaction times in the first column. These are the responses and response times to the presentation of nonsense names alone that had already been connected with a particular depicted object or action. First of all, it is interesting to note the conspicuous richness of response style with which these *S*s refer to the same stimulus object (itself good argument against symbolism!). The first five items: 'baby with a toy,' 'baby playing,' 'baby playing on floor,' 'baby going after his rattle,' and 'baby' are the various modes of reference to the same stimulus. But a more striking comparison is shown by the items in the second column, which constitute the responses made *immediately after* the trial recorded in the first column. Thus, 'baby playing,' with an RT of 1220 ms., is changed in the next succeeding trial to 'child' with an RT of 1000 ms. 'Girl skating' takes a response time of 600 ms. but 'roller skater' in the very next trial only 580 ms. 'Girl roller skating' and 'roller skating' [8] both require the same RT of 850 ms. Other examples may be seen in which RTs changed to a marked degree, and although some

[8] Results such as these have far-reaching implications for a psychological grammar as developed by Kantor (15, Chaps. 13–22, 16, 17, 18).

TABLE IX

SOME CHANGES IN THE PATTERNING OF THE VERBAL RESPONSES WITH THEIR CORRESPONDING REACTION TIMES IN MILLISECONDS

Verbal Response	RT	Same Verbal Response Changed to—	RT
Baby with a toy	520	Baby playing on floor with a toy	630
Baby playing	1220	Child playing	1000
Baby playing on floor	600	Baby on floor	520
Baby going after his rattle	570	Baby chasing a toy	730
Baby	660	Baby on its stomach	720
Man blowing horn	760	Bugle player	370
Soldier playing bugle	710	Man playing bugle	870
Soldier playing horn	760	Boy playing horn	490
Boy playing bugle	1170	Man playing bugle	910
Bugle boy	780	Bugler	650
Bugler	690	Man blowing bugle	620
Boy blowing bugle	1400	Boy with bugle	680
Bugle player	980	Bugler	1840
Chair	820	Straight chair	870
Chair	1670	The chair	1570
The straight chair	750	Chair	570
Girl roller skating	850	Roller skating	850
Girl skating	600	Roller skating	580

Note: All the above changes were made with *S* responding only to the nonsense syllable and with only the original exposure of the picture.

changed in an upward direction, most of them were found to be the same or shorter. Since the data are fragmentary, any more quantitative statement is impossible.

Argument against reduction of RT through 'practice effect.'—Since it might be argued that mere routine performance of a response *per se* is responsible for the reduction in the reaction time of that response, it might be well to recapitulate the findings discussed directly above. First of all, it will be noted that just as in the Reed, Leuba and Hyde, Book, and Bryan and Harter studies previously referred to, faster responses result from elimination of intermediate (symbolic) reactions. Table VII indicates the long RTs that accompany such intervening behavior. Furthermore, the morphological nature of the response needs to be considered; for note that the verbal patterning of successive reactions (Table VIII) may be changed without affecting reaction time one way or another. Evidence of shifting responses throughout the experimental session contraindicates serious consideration of a 'practice effect,' an argument better applied to a situation in which a prescribed unchanging response such as key pressing becomes automatized after a certain degree of practice. That such an explanation does not hold in the present instance is also shown by the failure of 17 *S*s to meet the criterion. Altogether, the above evidence argues against a practice effect explanation for

the reduction of reaction time. Apparently, not more performance but elimination of intervening response is the proper interpretation of the faster reaction times obtained here.

A casual semantic finding.—Another observational detail that made its appearance as incidentally as the variability of response patterning was of a 'semantic' nature, and shows the importance of an individual's manner of designating a stimulus linguistically upon his future dealings with that stimulus. This condition may be referred to as 'linguistic isolation,' which may be effective or ineffective depending on the behavioral consequences.

On various occasions during the experiment it was noted that a particular pair of slides was especially difficult for the *S*. This was apparent from the confusion of responses as well as the unusually long reaction times. It becomes evident, too, that responses rightly belonging to a particular object were made to an object on a different slide and *vice versa*. In other cases the same response or two very similar responses were made to both objects.

TABLE X

PARTIAL REPRODUCTION OF AN *S*'s RECORD ILLUSTRATING POOR 'LINGUISTIC ISOLATION'

First response is directly to picture; subsequent ones to nonsense syllable.

RUC-Girl-Skating *		WOJ-Man-Bugling	LEM-Baby-Playing *	
Response	RT	RT	Response	RT
Child running.....	610	450	Baby playing.....	430
Child running.....	900	900	Child playing.....	1210
Child running.....	1180	900	Child playing.....	1230
Child running.....	1020	750	Child playing.....	1050
Child running.....	900	950	Child playing.....	860
Child running.....	700	690	Child playing.....	680
Child running.....	930	830	Child playing— Child running**.	1290
Child running.....	970	570	Child playing.....	1970
Child playing.....	1840	700	Child playing.....	890
Child playing.....	770	690	Child playing.....	1430

* Semantic confusion is shown between two outside columns; center column is an example of a response not so confused and is included only for comparative purposes. The confusion between RUC-girl-skating and LEM-baby-playing and the result that long RTs are probably due to the similarity of references which this subject used for these two different stimuli.

** The confusion is all the more apparent in this trial with both responses given in quick succession.

Table X shows concrete examples of this 'semantic confusion' in the case of an *S* who happened to respond first with 'child running' to the RUC-girl-skating slide and 'baby playing' to the LEM-baby-playing slide when the pictures themselves were presented to him. His corresponding RTs to both slides are respectively 610 and 430 ms.

But compare the long RTs to both slides with continued exposure of the associated names only. These are seen to be considerably longer. When we note that the response patterning has been changed to 'child running' and 'child playing,' we begin to suspect that these responses have not been made distinct enough and therefore lead to confusion. This is more evident from data presented in the third column which show both responses, 'child playing—child running' made in quick succession to the 'baby' slide. The RTs for a stable response not so confused are given in the middle column for comparative purposes.

Apparently, the *form* of the reference is important in a proper linguistic isolation of a stimulus and in future perceptive, linguistic, effective and other interactions with it. Stimulus features (such as resemblance or contrast) may also have such results.

While experimental literature on the above point is unknown to the writer, it is interesting to relate these results to Watson's identification of the Freudian 'unconscious' with the unverbalized. In discussing a case commonly referred to as an incest complex, Watson states:

> The assumption I make is that the whole realm of her (the patient's) relationship to her mother has remained unverbalized . . . no hypothesis of the unconscious need be assumed—no hypothetical factor such as expression or repression need be lugged in. Talking to her in terms of the unconscious—of repressions, limitation of libido, incest complex and the like, leaves her cold. She has no realm of words to correspond with her behavior towards her mother except the conventional ones tolerated and extolled by society under the guise of the beauty of mother love. She need not and could not have ever conversed with herself about all this. She had no words, hence it never could have passed from the analyst's conscious to the unconscious (27, pp. 100–101).

In the light of the writer's formulation, Watson's case might be understood as a psychopathology resulting from a total lack of modes of reference in the linguistic isolation of her situation—a linguistic difficulty that prevented the patient's adjustment (i.e., her performance of other related responses) to her life circumstaucc.s.

6. *Evidence from Analysis of Action Potentials*

Psychologists have traditionally favored the laboratory investigations of behavior limited to the simpler movement responses involving gross spatio-temporal displacements of the organism. These are easy to handle and are also amenable to statement in precise, quantitative terms. However, it would seem that psychologists are just as much obligated to study the more difficult behaviors such as feelings, emotions, language and implicit activity ('thinking').[9] The latter are just as concrete and 'natural' as the grosser movements,

[9] Hilgard (14) has recently called attention to the current prejudice in favor of certain materials and techniques and has suggested one way out of the difficulty.

but are subtle actions requiring special techniques for their study. It would seem reasonable to suppose that all organismic responses *involve* (and not, 'are accompanied by') some sort of metabolic change, no matter how slight, a case in point being the inapparent action of the card player with a 'poker face' who may nevertheless be quite 'excited.' Proper instrumentation should show at least presence or absence of such organismic responses.

To aid in the study of the more subtle types of responses, a few psychologists are pioneering in the design of suitable instrumental techniques. Thus, Bartley (1) has extended oscillographic methods to the study of vision, and Davis (**8, 10**) has applied them to 'mental work' and complex learning reactions. The investigation of the more difficult problems by application of the best available techniques is carried on with profit and without pushing them aisde for future generations of students. Accordingly, in addition to the RT method discussed above the present study added the oscillographic technique as developed by Davis.

It will be recalled that the action potential method was introduced for the purpose of throwing additional light on the behaviors performed by the *S*s in the direct, test and final test series. Since certain relationships appeared in the reaction time data, would there be corresponding ones in a set of oscillographic data? Furthermore, there remained a possibility of finding diagnostic differences between criterion and non-criterion groups which would provide further justification for their separate treatment.

In order to deal numerically with the oscillographic data, the photographed records were measured in terms of a calibration of the amplifier made with a beat-frequency oscillator of known frequency and voltage. This procedure showed that the customary setting of the gain controls gave sensitivities of 1.25 microvolts per mm. on the recording paper.

It was then assumed that a shift in action potential from a high to a low level would perhaps be more indicative of change than one going in an upward direction from a resting level. Consequently, protocols for each *S* were examined and all declines of 2.5 microvolts or over per ¼ sec. unit were noted. A ratio was then computed of the actual number of declines to the total possible number of such declines per trial. In line with the above reasoning, this ratio, designated as D.Q. (decrement quotient), would be indicative of little action when low and greater degrees of action as it approached values toward unity.

When the average number of decreases in action potential was calculated for all the *S*s in the various series, the series are seen to be clearly separated in some cases (cf. Table XI). Since the distinction

TABLE XI

A Comparison of the Criterion and Non-Criterion Groups in Average Number of Decreases in Action Potentials under Various Experimental Conditions

Series	Criterion Group		Non-Criterion Group	
	Mean	SD	Mean	SD
Direct	9.44	4.14	8.06	3.29
Practice	18.13	4.90	17.41	5.67
Test	19.09	12.49	20.06	7.86
Final Test	7.03	4.00	7.76	2.24

applies to both groups (criterion as well as non-criterion), this index appears more indicative than the reaction times. Note, for example, that the criterion and non-criterion groups showed respectively means of 18.13 and 17.41 changes in the practice series during the *first* exposure of nonsense name and related object shown simultaneously (where the *S* was instructed to look first at the name and then at the pictured object in order that he might connect them). It is important to note that this is a behavior segment composed of two reaction systems, first, to nonsense name, then to pictured object on slide. The similarity of these series and the immediately succeeding ones in which only the nonsense name was exposed is quite conspicuous; but it is more important to note that the test series (first, unpracticed and so, indirect) show averages a little more than twice those shown for the direct series (i.e., original responses to the illustrated object alone). The final test series in which the indirect response was well practiced shows a great decrease with a value approximating the original direct response. This would suggest that the well-practiced indirect act has become similar to the direct. This approximation of the final test to the direct holds for both the case of the criterion group and for the non-criterion group. Tests of significance were not applied because these figures are complicated by temporal features of the responses which have not been taken into account, although one could argue that one reason for the longer times here is that more is happening in the first test series. Some responses were long and others short. It was for this reason that the temporal variable was ruled out by expressing the decrease in action potentials as a ratio of total decreases to total possible number of decreases—the D.Q.[10] measure previously referred to.

[10] Since the validity of this measure may itself be in question inasmuch as the D.Q. might be a function of the overall decline in action potentials, the following validation test was performed. For 100 trials selected at random, final level in action potentials was subtracted from initial level and the resulting figures were correlated with figures expressing the corresponding number of decreases for those trials. A correlation coefficient of + .212 was derived, which is not significant at the five percent level. There is, therefore, little reason to question the validity of the D.Q. measure.

Tables XII and XIII summarize the refined data secured for the groups separated on the basis of their reaction time performance. Table XII shows an increase in the number of changes in action potential of test over the direct series. This result is consonant with the RT findings. Testing the differences among the direct, test and final test series gives a χ^2 of 57.70 for the criterion group, which is significant at the one percent level.

TABLE XII

DECREASES IN ACTION POTENTIAL FOR GROUP OF 33 *Ss* MEETING CRITERION

D.Q. expresses a ratio of actual number of decreases to total possible number of such decreases.

Series	Mean D.Q.	SD	χ^{2*}	Level of Significance
Direct	.27	.100	57.79	1%
Test	.29	.090	57.79	
Final Test	.25	.090		

Note: The mean D.Q. rises in the test situation but declines in the final test to a level below the mean of the direct series indicating fewer decreases in action potential toward end of practice.

TABLE XIII

DECREASES IN ACTION POTENTIALS FOR GROUP FAILING TO MEET CRITERION

Figures express a ratio of actual number of decreases to total possible number of decreases.

Series	Mean D.Q.	SD	χ^{2*}	Level of Significance
Direct	.26	.100		
Test	.25	.090	21.34	5%
Final Test	.31	.110		

Note: The direction of the mean D.Q. is here seen to be in a direction opposite to that of the criterion group. The test series shows a slight decrease but the final test series indicates a greater frequency of increased muscle action potentials over the first series.

* χ^2 in both the criterion and non-criterion cases was obtained by classing the respective measures in step intervals, which in both cases gives 27 degress of freedom.

More interesting results are shown in Table XIII for the group failing to meet the RT criterion. It will be noted that the direct and test series are close together in their mean D.Q.s which are, respectively, .26 and .25. The final test series (following considerable practice) show a decided increase as indicated by the mean D.Q. of .31. This is in contrast to the criterion group that showed a drop at this point in the experiment. A test of the differences of the individual measures for the experimental series of the non-criterion group gives a χ^2 value of 21.34; this is not significant at the five percent level. In summary, the χ^2 tests indicate that for the action potential data of the criterion group sample there is significant

variation in the D.Q.s of the various series. The measures for the non-criterion group, on the other hand, are rather homogeneous with insignificant variation between the experimental categories.

Since the χ^2 is after all a single number characterizing a set of data, it may be worthwhile to examine the original data for significant trends. It is apparent that the action potential D.Q.s diverge sharply for the two groups, the criterion group showing a drop from test to final test while the non-criterion group shows a rise in D.Q. in a comparison of the first and last test series.

In general, the increase in action potentials for the group failing to meet the criterion would suggest difficulty in attempts to comply with expected requirements. In this connection it is interesting to note that Davis (9, p. 157) found increase in action potentials with increasing difficulty of task. Such factors may have operated in the case of our non-criterion *S*s.

Additional confirmation of the hypothesis that the easier, direct response is indicated by fewer action potential decreases may be derived from data shown in Table XIV. These were action potentials

TABLE XIV

ACTION POTENTIAL DATA FOR 13 *S*s OF THE EXPERIMENTAL GROUP THAT WERE GIVEN 20 TRIALS OF CONTINUED PRACTICE BEYOND, AND ONE WEEK FOLLOWING, THE ORIGINAL SESSION

Action potentials were recorded for the 20th trial to the nonsense syllable and for the 21st to the pictured object.

Series	Mean D.Q.
Final Test	.311 ± .100
Direct	.323 ± .083

from oscillographic records secured in the trial of a series given one week following the experimental session. During the whole series these responses had been made to the nonsense name presented alone without any simultaneous exposure of the correlated object. The 21st trial consisted of a response made to the objects depicted on the slide, a step included so as to compare the two responses for possible similarity.

The final test series for 13 *S*s randomly selected from the criterion group that had been given this continued practice of the originally indirect response shows a mean D.Q. of .311. Note that this is below the value of the D.Q. of .323 derived for the next succeeding trial made to the pictured object as in the original direct series. The significant point here is that the indirect response so unlike the direct at the beginning of the experiment approximates it in both action potentials as well as reaction time, as was previously indicated. The fact that the non-criterion group fails to show a corresponding de-

crease is strong evidence that the response under observation was not of a simple 'reflex' type that mere practice alone would somehow ultimately reduce in both reaction time and action potentials.

Summary and Conclusions

The present experiment, deriving from a consideration of the traditional symbolic and the more recent interbehavioral theories of language, suggested setting up two distinct linguistic situations corresponding to them.

> Situation A. *Direct or non-substitutive language action* in which the *S* is presented with a series of slides exposed on a screen in front of him; these represent common objects or events. He is instructed to answer as quickly as possible to each slide in turn.
>
> Situation B. *Substitutive language action:* The *S* is now shown the same slides as in *A* but with certain nonsense names associated with each. After one practice series, the nonsense names are exposed, and the *S* is required to answer as quickly as possible with the name of the corresponding object. This substitutive action is the essential difference between Situations A and B.

Beyond this point, the *S*'s reactions to successive practice series of slide presentations were observed, recorded and analyzed in order to determine if the substitutive reaction becomes a direct one. Reaction time and oscillographic records were obtained from 50 *S*s treated in accordance with the above procedure.

1. After a number of practice series in Situation B, which practical considerations necessarily limited to a maximum of 20 trials, 33 *S*s (the criterion group) approximated their RTs of Situation A. Seventeen *S*s failed to do so. For the criterion group a CR of 2.68 was obtained between the direct response in Situation A and the final response in Situation B, and a CR of 7.29 on a test of significance of mean differences between the *first* such indirect response in Situation B and the final one.

2. Results from RT data of 13 *S*s given continued practice for 20 trials one week following the original session show an even closer approximation of direct and final test RTs. These were from both the criterion and non-criterion groups.

3. A check experiment with four *S*s, which shows an initial advantage inherent in the pictured object (over the nonsense syllable stimulus), tends to enhance the results obtained in the final test series of the criterion group.

4. The non-criterion group showed somewhat different results. The means for their direct and final test series show significant differences in RT (CR = 4.452), which means that these *S*s did not perform the same after much practice in the indirect situation as in the direct part of the experiment. This finding may be interpreted to signify that their indirect (symbolic) responses remained symbolic

responses throughout the experimental session. These divergent results of criterion and non-criterion groups were scrutinized for further differentia.

5. Action potential data show correspondence with RT findings. Comparison of criterion and non-criterion groups in average number of action potentials under various experimental conditions shows about twice as many decreases in the practice and test series as in the final test and direct series.

6. Computation of decrement quotients (ratios of total number of decreases to total possible number of such decreases) for the three experimental categories yielded a χ^2 value significant at the one percent level of significance. Similarly, a χ^2 value failing to meet the five percent level of significance was obtained from the same measures of the non-criterion group, thus indicating a greater homogeneity of the several measures in the non-criterion group as compared with their variability in the criterion group.

7. Perhaps more important still is the divergent performance of the criterion and non-criterion groups, with criterion *Ss* indicating a reduction in the number of sizable action potential decreases while the non-criterion group showed an opposite trend from beginning to end of experiment.

8. Results from the action potential records of the same 13 *Ss* mentioned in (2) showed that the test response, so unlike the direct response at the beginning of the experiment, resembles it in action potential characteristics as well as RT.

9. Finally, in the light of further observations that showed numerous instances of variability in verbal patterning without a consequent increase in RT, together with other cases of extremely long RT in verbalized segments involving two reaction systems, and supported by experimental findings of investigators in related fields pointing to the elimination of intervening symbolic responses with practice, the above results are related to the hypothesis that an indirect or substitute response may become a direct referential act as affirmed by interbehavioral language theory.

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FLUCTUATION OF SENSATION OF LIMINAL VISUAL STIMULI *

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The study has been divided into two parts: Part I is concerned with the effects of introducing eye-movement as an independent variable; Part II is concerned with the relationship of the total fluctuation phenomenon with stimulus-intensity.

PART I

In order properly to define the limitations of this study, we must risk the following categorization. If a sense organ is in the state of readiness-of-response to a constant stimulus of its own modality, three levels of activity dependent on stimulus-intensity may be expected when the stimulus is applied. These are:

1. If the stimulus is below the intensity threshold of the sense organ, no response will obtain. It is assumed that the stimulus is insensible as far as the organism is concerned, and that the organism remains unaware of the stimulation.

2. If a constant stimulus of liminal intensity is applied to the organ set described, the fluctuation type of response follows. Part of the time it is insensible to the organism and part of the time it is sensible. This is the phenomenon of fluctuation of sensation, commonly called the fluctuation of attention.

3. If the stimulus is of an intensity well above the threshold of the sense organ, the stimulus may be sensible to the organism at all times.

Problem

From the time of the first specific study of the problem of fluctuation by Urbantschitsch (**9**) who believed the basis for fluctuation was in the 'perceptive ability' of the nerve, experimenters have been in disagreement with regards to the location and cause of the fluctuation phenomenon.

Lange denied the fatigue theory of Urbantschitsch and concluded that the phenomenon is a fluctuation of active apperception which produces a periodic assimilation of the image with sensation. Mün-

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sterberg (**6**) disagreed with Lange, concluding that causation lay chiefly in the muscles of accommodation and fixation. He was refuted by Pace (**7**) who found little effect on fluctuation when the eye was treated with atropin.

The results of these experiments precipitated a series of experiments with conflicting results concerning the location of the phenomenon and its relation to body rhythms.

Pace (**8**), using the negative after-image in conjunction with fluctuation, came to the conclusion that retinal adaptation has something to do with the disappearance of the image.

Ferree (**2**) found that eye-movements break up adaptation. But this was denied by Liddell (**5**) who photographed the eye-movements during fluctuation and found little relation between the two.

Guilford (**4**) also photographed eye-movements and found that: (1) local retinal adaptation is a factor in the disappearance of lights during fluctuation with liminal stimuli; (2) winks and refixations are occasional factors favoring the return of liminal light which has disappeared; (3) eye-tremors may furnish a constant factor for visibility; (4) winks and refixations often bring about momentary disappearance of liminal light; (5) there are other factors than eye-movement which promote the return of liminal light which has disappeared.

The present experiment is concerned with the effects of introducing eye-movement as an independent variable in the fluctuation situation.

Apparatus

The constant stimulus used in the experiment was a faint gray trace made by a modified form of the Masson disk. A drawing of a portion of one of the disks is shown in Fig. 1.

The disks were made of white card-board, and the dots were punched from a sheet of dark blue paper. Despite the fact that dark blue paper was used, the trace appeared as a faint gray. Six disks were used having 2, 2½, 3, 3½, 4, and 5 dots respectively. Testing was done in a photographic dark room adjoining the experimental laboratory. The disks were illuminated with a 40-watt bulb in a reflector, the only illumination in the room. The intensity of the light on the disk was kept uniform and was checked with a photographic exposure meter before and after each testing period.

A combination monoscope and head rest served to restrict the visual field.

Two pens of a polygraph recorded the results. One pen was connected with a telegraph key and was depressed by *O* during sensation of the stimulus. The second pen recorded time in seconds.

Preliminary experiments had shown that only three types of eye-movement could conveniently be tested. These were: (1) No eye-movement, in which *O* kept his eyes fixed on one point for the duration of the sample; (2) limited eye-movement, in which he was relatively restricted in the movements of his eyes; and (3) unlimited eye-movement, where *O* was given almost complete freedom of eye-movement. The latter two constitued the independent variable while the first served as a control.

1. *No eye-movement.*—This method (the control) was used one-half the time by both groups. The *O* was instructed: "Fix your eyes here" [indicating an imaginary point 3 to 4 mm. above the gray trace]. "Keep looking at that one spot and depress the telegraph key as long as you can see the gray line."

2. *Limited eye-movement.*—This method was used by group I and was alternated with the control method. The *O* was instructed: "Start out by fixing your eyes on the point above, but as soon as the trace fades, shift your eyes to a point an equal distance below. When the trace fades again shift back to the point above. Shift every time the gray line fades on you. Depress the telegraph key as before."

3. *Unlimited eye-movement.*[1]—Group II used this method which was also alternated with the method of no eye movement. The *O* was instructed: "Fix your eyes on the point above, but after two or three seconds shift them to an equal distance below the trace. After two or three seconds shift back to the point above. Keep shifting back and forth between the two points every two or three seconds. Depress the telegraph key as before."

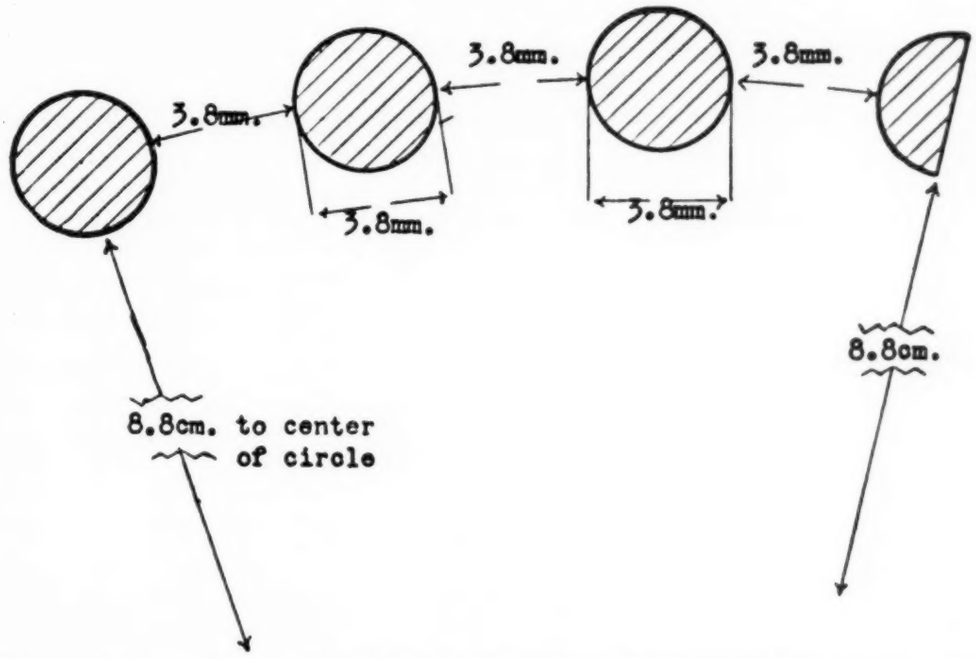

FIG. 1. Diagram of a portion of the stimulus disk. The diagram shows the size and relative positions of the dots on the disk which had a stimulus-intensity of three and one-half dots.

The first time *O* used the method of no eye-movement he fixated on a point above the trace; the second time his fixation point was below the trace. In alternating this procedure with the method of eye-movement, no particular retinal area was used more than any other area during the testing period.

A diagramatic reconstruction of the retinal area involved was obtained by using the dimensions of the schematic eye. It was found that the vertical dimensions of the retinal image of the trace fell within the *fovea centralis* and its lateral dimensions extended into the retina proper. This would suggest that the bulk of the sensation was concerned with cone vision.

The results of the first and second half-hours of the preliminary experimentation, in which testing was broken up into eight to twelve samples lasting two to three min. each, showed no fatigue effects when compared by the method of analysis of variance. Despite the lack of reliable differences between the periods in question, sample time was reduced to approximately one min., while the number of samples taken in a given hour was increased to 16.

RESULTS AND DISCUSSION

The two groups of data yielded by the method of no eye-movement (during the first sample *O* fixated on a point above the trace;

[1] The term 'unlimited eye-movement' is used to contrast this method with the method of limited eye-movement. Actually eye-movement is relatively restricted in both instances. The experiment could have been improved considerably if eye-movement could have been checked photographically, but our laboratory does not have these facilities.

during the third sample *O* fixated on a point below the trace) which was used as a control, were combined after first checking to see if they were statistically homogeneous. Comparisons with the methods of eye-movement were made with this combined group.

The means obtained from the two methods (the method of no eye-movement and the method of eye-movement) and the critical ratios of their mean differences are found in Table I.

TABLE I

TABLE OF RESULTS COMPARING THE METHOD OF NO EYE-MOVEMENT WITH THE METHOD IN WHICH EYE-MOVEMENT IS USED*

	Dots	No Movement		Movement		Critical Ratios	
		Observed	Disappeared	Observed	Disappeared	Observed	Disappeared
FD	4	19.23	2.64	−15.84	−2.33	1.27	0.80
	3½	15.34	3.84	−13.47	−2.43	0.12	3.30
	3	13.46	4.50	−10.30	−3.64	1.73	0.91
	2½	14.03	5.98	−11.53	−4.67	0.48	1.29
	2	6.94	6.42	− 5.72	−4.78	2.91	3.88
CL	4	13.51	1.39	−13.13	−1.28	0.27	0.19
	3½	8.00	1.45	9.00	−1.30	0.80	0.35
	3	6.87	2.34	7.21	−1.66	0.36	2.29
	2½	4.60	2.22	5.33	−2.00	1.40	0.70
	2	3.98	3.08	− 3.92	−1.99	0.17	2.11
BP	5	23.02	1.00	−21.99	1.08	0.12	0.41
	4	8.05	1.95	− 6.92	−1.40	1.03	2.25
	3½	3.55	1.48	3.81	−1.26	1.17	0.95
	3	2.53	1.43	− 2.48	−1.17	0.24	2.97
	2½	2.88	1.80	− 2.33	−1.35	1.88	3.24
	2	1.76	1.66	− 1.51	−1.27	2.24	3.52
SF	5	7.14	0.56	—	—	—	—
	4	4.52	0.89	40.42	−0.46	7.52	2.20
	3½	3.25	1.11	16.33	−0.84	2.65	1.49
	3	3.60	2.01	14.17	−1.95	5.39	0.17
	2½	2.65	2.70	7.07	−0.91	7.36	5.62
RH	5	7.84	1.03	—	—	—	—
	4	9.77	1.28	84.87	1.31	1.34	0.11
	3½	7.50	1.21	7.92	−1.18	0.62	0.30
	3	13.61	0.99	16.83	−0.86	1.88	1.64
	2½	9.10	0.87	11.95	0.93	2.36	0.77

* The mean time of observation and disappearance is given in seconds. Each mean represents from 8 to 16 samples, as sample is defined in the last paragraph under procedure. The method of no movement is the control method with which the method of movement is compared. The minus signs indicate a decrease of mean time as compared with the control. The first three *O*s (FD, CL, and BP) used the method of limited eye-movement; the last two (SF and RH) used the method of unlimited eye-movement.

The data in Table I show several characteristics. Some of these characteristics show themselves as persistent effects, others as general tendencies.

As stimulus-intensity increases the duration of the sensible period tends to increase while the duration of the insensible period tends to decrease. This effect is independent of eye-movement and holds for all *O*s except RH.

The data of RH tend to refute the conclusions of the entire experiment. Preliminary testing did not indicate the need for preparing for such an occurrence. At the time of the present experiment some testing was being done with the Seashore pitch discrimination record. The results of RH on the latter showed a similar erratic tendency. This particular *O* showed an enormous variation on the liminal hearing threshold, as indicated by the Seashore results while 10 trials were made. Unfortunately, his data on the visual limen were not checked until a later period. Consequently there was no way of finding if a correlation existed between shifts of the visual and auditory limen.

With the exception of SF, few reliable differences exist between the eye-movement and no eye-movement methods.

Group II (unlimited eye-movement) shows persistently higher means for the eye-movement method when compared with the method of no eye-movement when the sensible periods are compared. The results of this same comparison with group I (limited eye-movement) are not clear; 12 of 16 means of the no eye-movement method are greater than the eye-movement means. The results seem to indicate that the method of no eye-movement tends to give higher means than the method of limited eye-movement.

When a comparison is made between the insensible periods of the eye-movement methods with the control, the *O*s in group I have persistently lower means for limited eye-movement, the ratio being 15 : 1. The same effect seems to persist with group II, the ratio being 3 : 1. Introduction of eye-movement of either type caused a general decrease in the average length of the insensible period.

In listing the results of the five *O*s who observed the constant liminal stimulus under three experimental situations (no eye-movement, both groups; limited eye-movement, group I; unlimited eye-movement, group II) several effects are demonstrated. These are:

1. The duration of the sensible period tends to increase with increase in stimulus intensity while the duration of the insensible period tends to decrease. The effect obtains independently of eye-movement.

2. Reliable mean differences between any two methods are the exception, not the rule. Despite this lack of reliable differences, certain persistent effects show themselves. These persistent effects are:

a. When unlimited eye-movement is used, the means of the

sensible periods are always higher than the same means obtained with no eye-movement. When limited eye-movement is used, the reverse tends to be true: the introduction of limited eye-movement tends to reduce the average length of the sensible period.

b. When unlimited eye-movement is used, the means of the insensible periods tend to be lower than the same means obtained when no eye-movement is used. This effect persists when limited eye-movement is used.

Discussion and Interpretation

The effects of introducing eye-movement into the experimental situation offer some suggestion as to the possible causation of the phenomenon of fluctuation. In order to make a meaningful interpretation of the results, it is necessary to bear in mind the relationship of the retinal areas involved.

When no eye-movement is used, the retinal area presumably remains constant. The resulting fluctuation involves only one particular section of the retina.

When eye-movement is introduced, two relatively distinct retinal areas are involved.

When limited eye-movement is used, the first retinal area is held until the stimulus is no longer sensible; the *O* then shifts to the second area until sensibility again fades; whereupon *O* shifts back to the first area, and so on. Since the duration of the sensible period is greater than the duration of the insensible period, except in rare occasions when the stimulus-intensity reaches the liminal value, it would be expected that the duration of the insensible period should remain constant. The area has had ample time for readaptation, and the duration of the insensible period should be only as long as it takes the eye to make the necessary fixation.

In the experimental results this does not hold true. The length of the period of insensibility is some inverse function of stimulus-intensity regardless whether eye-movement is used or not.

When unlimited eye-movement is used, the same two areas are involved as in the case of limited eye-movement. Now, however, the *O* does not wait for the image to fade, but shifts to the opposite area before fading can occur. As would be expected, due to the partial elimination of the fatigue element, the sensible period should be longer than in the case where no eye-movement is used. This was the case in the experimental results.

The duration of the insensible period with the use of unlimited eye-movement is not so predictable. On the one hand it might be expected that the duration of the insensible period should be the

same as that obtained with no eye-movement for the previously stated reasons. On the other hand, it might be expected that the duration should be shorter since the results of all the situations show that the period of insensibility decreases as the period of sensibility increases. Unfortunately, the data are too inadequate to answer the implied question. By-and-large the duration of the insensible period seems to be more closely related to the stimulus-intensity than to the duration of the associated sensible period.

Several interpretations of the effects of introducing eye-movement suggest themselves.

It was found that the duration of the sensible period increased with the increase in stimulus-intensity, while the duration of the insensible period decreased. An analogous situation was found by Adrian and Zotterman (1) in the relation of the nerve impulse to its succeeding refractory period. The fluctuation of sensation may be a higher order of the responses that are intrinsic to the end-organ. This statement is made with reservation, since the phenomenon of fluctuation has not been found in the end-organ experiments as yet.

It is more likely that the nature of the fluctuation is similar to the phenomenon of muscle tonus as described by the physiologists. In that case, the tonus is thought to result from the fact that all the muscle fibers are not in activity at one time, there being a constant shifting of the load from one group of fibers to another. Because liminal stimuli are used in conjunction with the fluctuation problem, there is reason to believe that all the neurones in the effected retinal area are not active. It is quite possible that there is a shifting effect, similar to the description of muscle tonus, with one set of end-organs (and nerve-fibers) carrying the load until the point of fatigue is reached, at which time the load is assumed by another set. In this case the insensible period would occur when a condition is reached where none of the fiber-sets was in a state of readiness of response to the stimulus, the insensible period persevering until one of the fiber-sets was again able to respond.

If this interpretation is true, it would mean that the fluctuation is a characteristic of the nerve-bundle and not of the individual fibers. This is not illogical, since the all-or-none law is a characteristic of the nerve-fiber but not necessarily the nerve-bundle.

The second result found was that reliable mean differences between any two methods of stimulus observation were the exception, not the rule. Despite the lack of reliable differences, certain persistent effects showed themselves.

The first persistent effect observed was that, when unlimited eye-movement was used, the means of the sensible period were always greater than the same means obtained with no eye-movement. This

is natural. A particular field of sense-endings is not used to the point of complete fatigue, but has a rest period every two or three seconds. In the case of unlimited eye-movement the retinal area is given periodic rest and, as a consequence, is able to function for longer periods of time.

With the use of limited eye-movement the results are not so readily explained. Here it was found that the means of the sensible period tended to be less than similar means obtained with no eye-movement. Ordinarily it would be expected that the same result, but to a lesser degree, should obtain as in the case of unlimited eye-movement. This was not true. In 12 out of 16 cases the means of the limited eye-movement are smaller than the means of no eye-movement. There seems to be no logical explanation for this discrepancy. It may be that in this circumstance something similar to the treppe effect in muscular contraction occurs. In the case of limited eye-movement, two distinct retinal areas are involved, while in the case of no eye-movement, only one area is used. In the latter case it may be possible that the area gets 'warmed up' to the stimulus and once 'warmed up' functions at the peak of its efficiency. In the case of limited eye-movement it may be that the area is always in a state of 'warming up' and in consequence never gets to function at peak efficiency. As soon as the period of insensibility appears the retinal area is shifted and the functional activity remains in the state of 'treppe.' This explanation is given hesitantly because it occurs in only 12 out of 16 cases. It was suggested by a laboratory observation that the first few observations of a given sample tend to be of short duration.

The second type of persistently appearing results concerns the means of the insensible periods. When unlimited eye-movement was used, these means tended to be smaller than the same means for no eye-movement. The same was true when limited eye-movement was used. The introduction of eye-movement caused a shortening of the average period of insensibllity. The effect held for 21 of 24 comparisons.

It was in this part of the results that the most pronounced effect of eye-movements was demonstrated. From this it follows that any increase in visual efficiency due to the effects of introducing eye-movement will tend to show itself in the period of insensibility. The increase in visual efficiency will consist primarily in the reduction of the average length of the insensible period. The theory of nerve-fibers acting in groups seems best to explain this. The introduction of eye-movement increases the affected retinal area. The increase in retinal area indicates an increase in the number of fiber-sets participating which, in turn, increases the likelihood of at least one group

being ready to act. This, of course, implies a reduction in the length of the insensible period.

Summary

The problem of fluctuation of sensation was investigated with the use of a modified form of the Masson disk. Records were made of the effects of using no eye-movement, using limited eye-movement, and of using unlimited eye-movement. The results showed:

(1) The average period of sensibility of the liminal stimulus increased with increase in stimulus-intensity. Correspondingly, the average period of insensibility decreased with increase in stimulus-intensity.

(2) Few reliable differences in means between any two methods of observing the constant stimulus were obtained, but nevertheless certain persistent effects occurred. These were:

(*a*) The introduction of unlimited eye-movement caused an increase in the average period of sensibility when compared with the situation where no eye-movement was used. The use of limited eye-movement tended to show the opposite effect.

(*b*) The introduction of eye-movement of either type caused a decrease in the average period of insensibility when compared with the situation where no eye-movement was used.

PART II

Purpose

Previously the relation of stimulus-intensity to either phase of fluctuation has been considered separately. Guilford (4) states that the relation of the sensible period to stimulus-intensity follows the phi-gamma function. He also adds that the duration of the insensible period is a function of stimulus-intensity, without saying what that function is.

It would seem that a more logical interpretation would embody the relationship of the total fluctuation with stimulus-intensity.

The results of Part I seem to indicate that either phase of the fluctuation may be dependent on the other, and that the relationship between the two may be dependent on stimulus-intensity. Accordingly, this second part of the experimental results will be devoted to the relationship of the sensible period, the insensible period, and the stimulus-intensity taken simultaneously.

In working with the sensory end-organ, Adrian and Zotterman (1) found a somewhat analogous situation. Following the application of a constant stimulus, the end-organ discharged an impulse. This

discharge of the impulse threw the cell into a refractory period. Recovery from the refractory period was gradual, but when the end-organ had recovered sufficiently, another impulse was discharged. The two experimenters found that stimuli of high intensity caused a discharge of the impulse early in the recovery period, while low-intensity stimuli caused a prolongation of the period of no response. This is essentially the same as that shown in the results of the present experiment, i.e., the stronger the stimulus the shorter the period of insensibility. There is, of course, one big difference: The refractory period lasted, in general, less than 0.1 sec. The impulses were discharged from the end-organ at rates varying from 5 to 100 per sec., depending on the intensity of the stimulus. When a constant stimulus was applied to the end-organ for a period of several seconds, the results showed a constant adaptation to the stimulus, the number of impulses per sec. being on a constant decrease. Fluctuation, in the sense of this experiment, did not occur.

The results of Adrian and Zotterman suggest that there is as much relationship between the active and the inactive periods of nerve excitation as there is between either of the two periods and the strength of the constant stimulus. This is to say that the number of discharges per second is dependent both on the refractory period and the stimulus-intensity. There is reason to believe that the nature of this interrelationship is similar in the case of the problem of fluctuation.

Following the belief that a relationship existed between the combined phases of fluctuation and stimulus-intensity, several methods of expressing this supposed relationship were tried before a successful method was found. This method follows:

When the mean duration of the sensible period was divided by the mean duration of the insensible period and the results plotted against the corresponding stimulus-intensity, a positively accelerated curve resulted. Furthermore, when the logarithm of the product was plotted against the corresponding stimulus-intensity a linear curve obtained. From this it can be assumed that the variation of the non-linear curve can be approximated by the equation

$$y = be^{ax} \text{ in which:}$$

y is the mean duration of the sensible period divided by the mean duration of the insensible period.

x is the stimulus-intensity.

e is the base of natural logarithms.

a and b are constants which vary from O to O.

Curves were plotted by the method of least squares for all data, separately and in various combinations. Three curves are included.

The curves obtained tend to substantiate a laboratory observation that curve forms were not affected by combination. Curve SF (Fig. 2) shows the results obtained in combining homogenous groups (the method of no eye-movement). Curve CM (Fig. 3) gives the

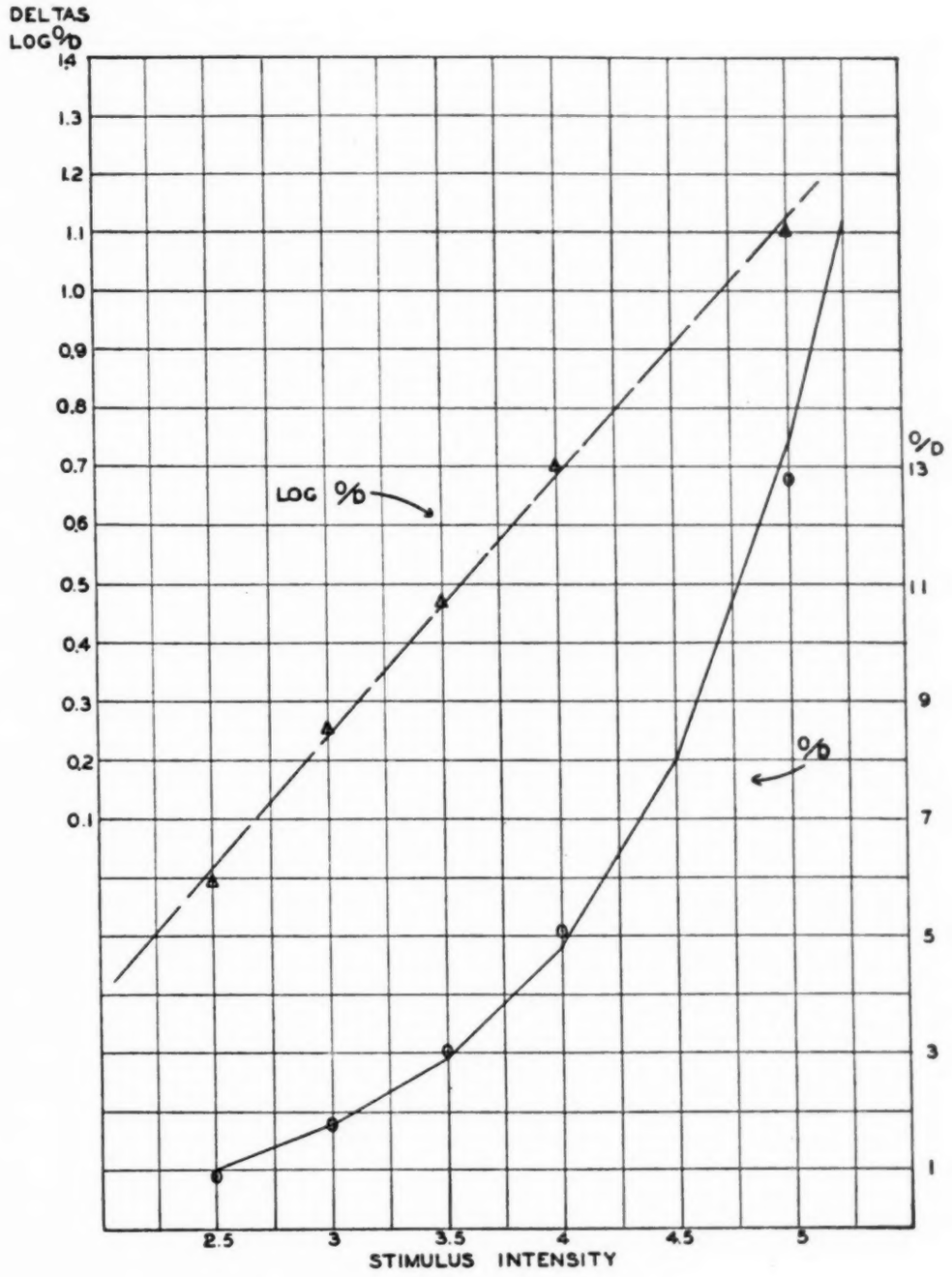

FIG. 2. Curve-fitting results for SF.

results obtained when the methods of no eye-movement and limited eye-movement were combined. Curve TComb. (Fig. 4) represents the results obtained when the unweighted means of all *O*s (except RH) were plotted against stimulus-intensity.

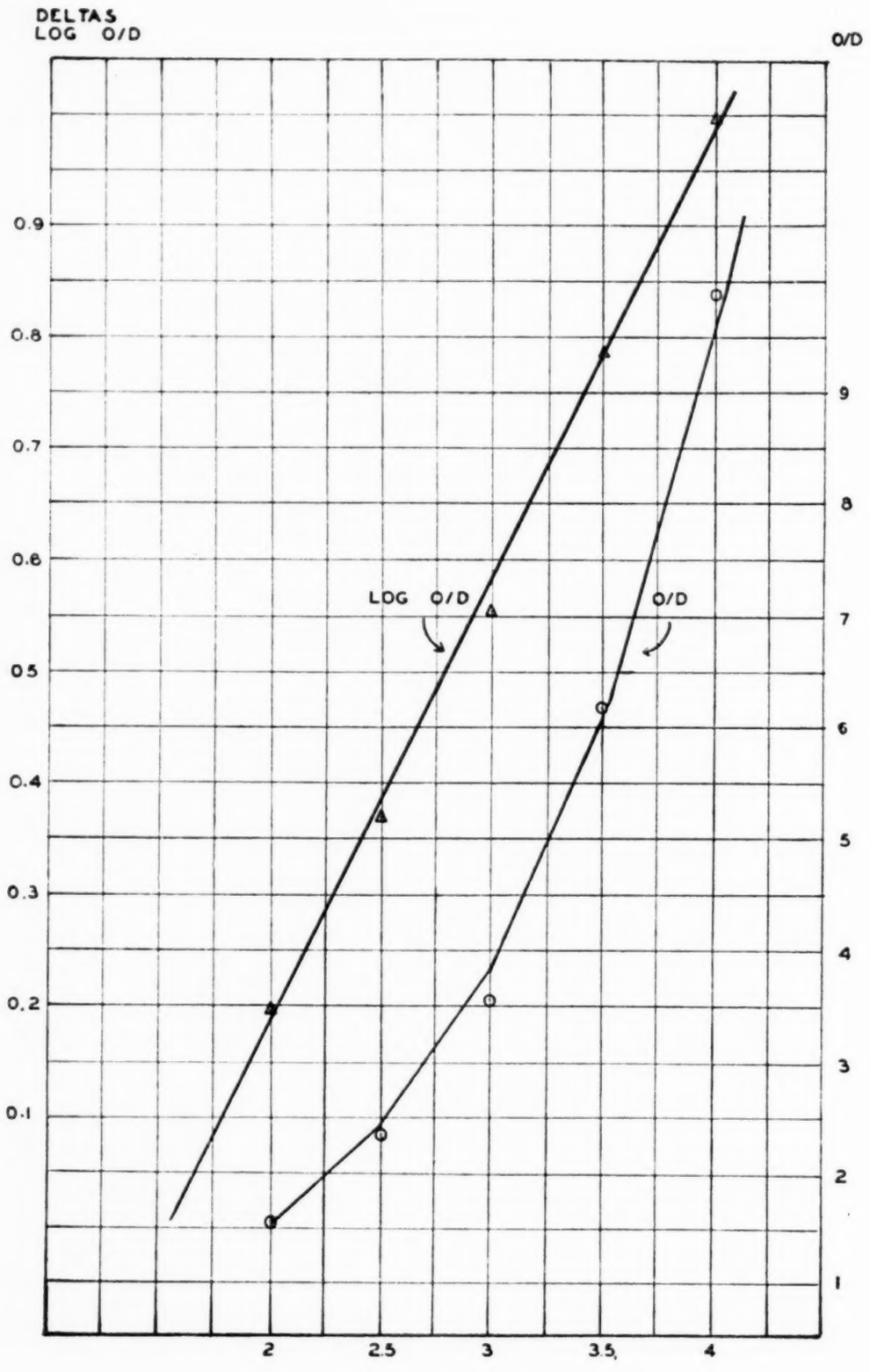

FIG. 3. Curve-fitting results for CM.

INTERPRETATION

There is little interpretation to be made of the results of the second part of the experiment.

The results seem to bear out the hypothesis that the durations of the sensible and the insensible periods are dependent on one another, and that the nature of their interrelationship is dependent on stimulus-intensity.

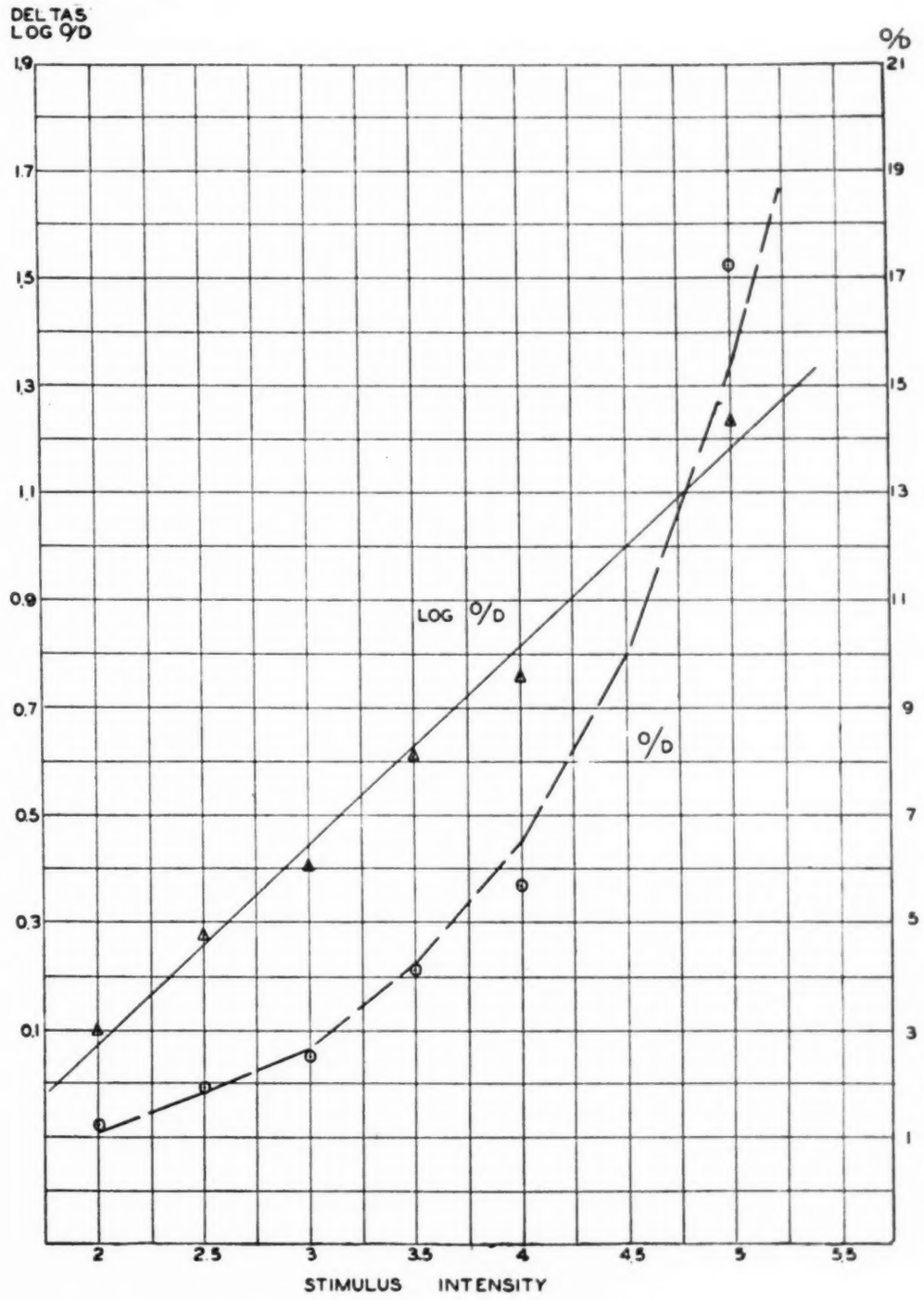

FIG. 4. Combined curve-fitting results.

Support is brought to this hypothesis by the fact that the three components (duration of both phases of fluctuation and stimulus-intensity) were reducible to the form of a simple function when they were expressed simultaneously.

The appearance of the log curve seems to suggest the existence of some physiological mechanism operating throughout the whole fluctuation phenomenon.

The results of Part II also suggest that perhaps it is erroneous to consider either phase of fluctuation independently, that the two are so intimately related as to demand that they be expressed together. It must be remembered, however, that the results were obtainable from only four of the five *O*s used in the final experiment. Consequently, all interpretations must be made with reservation.

Summary

A simple expression was sought that would explain the nature of the interrelationship of three of the elements of the fluctuation phenomenon: duration of the sensible period, duration of the insensible period, and stimulus-intensity. It was found that with four out of five *O*s this interrelationship could be expressed by the equation:

$$y = be^{ax}$$

The results, while obtainable from only four of five *O*s, suggest that the sensible period and the insensible period of fluctuation are dependent on one another, and that the nature of their interdependence is qualified by stimulus-intensity.

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VISUAL ACUITY AND DISTANCE OF OBSERVATION

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Introduction

Visual acuity is usually defined as the reciprocal of the minimal perceptible angle, measured in minutes of arc (**12**, p. 775). It is implicitly assumed that this definition makes acuity practically independent of the distance between object and observer.

"From this point of view one considers it irrelevant whether a test be made with relatively large objects at a large distance, or with small objects at a shorter distance. Indeed, in the proposal concerning uniform measurement and specification of visual acuity drawn up by Hess for the International Congress of Ophthalmology (1909) there is no requirement that a given distance be adhered to; the test-board values are calculated for a distance of 5 meters, but anyone who wishes to work with 4 meters need only, according to Hess, multiply the values calculated for 5 meters by 0.8 in order to be able to use the same test board" (**14**).

The data in the literature fail to support this simple general assumption. It would appear, rather, that with change of distance visual acuity may decrease, remain constant, or increase, depending on the configuration of stimuli involved and on the method of presentation (**4, 8**).

The older data cover only short distances. Aubert, with Foerster a pioneer in this field, ascribed the decrease of acuity observed in the Aubert-Foerster phenomenon to an indirect effect of accommodation (**1**, p. 244). In consequence, subsequent research was largely confined to distances at which changes in accommodation might be expected to play a part.

The explanation proposed by Aubert, however, has been questioned by several of the more recent investigators. Jaensch found that the Aubert-Foerster phenomenon persists even when accommodation is held constant by compensating lenses. He concluded in favor of a theory involving distribution of attention (**13**, pp. 321–352).[1] Jacobsson, on the basis of Jaensch's data and of some further

[1] By far the most thoroughgoing discussion of the Aubert-Foerster phenomenon is that of Jaensch. On the basis of his data on the A.-F. phenomenon and of a considerable number of related observations Jaensch concludes: (1) The essential factor in the production of the A.-F. phenomenon is apparent size. The larger the apparent distance between fixation point and peripheral object, the less adequately can attention extend from the fixation point to that object (**13**, pp. 96–109). (2) The A.-F. phenomenon and the Koster phenomenon are both manifestations of the same basic principle. In the K. phenomenon, "a bright object appears in micropsia brighter, a dark one darker, and illumination appears to increase" (**13**, p. 139). Jaensch

data of his own, concluded likewise in favor of a central theory, though not necessarily of Jaensch's (**14**). Freeman pointed out that Aubert's explanation is hardly consonant with the fact that, for a given change in distance, acuity may be increased or decreased according to the task set the observer (**9**, p. 293).

If the discrepancies from the principle that visual acuity is independent of distance are due to central mechanisms, it is to be expected that such discrepancies will be observed for large distances as well as small. Data involving large distances thus acquire both factual and theoretical significance. The only such data known to us are those of Forbes (**5**) and of Forbes and Holmes (**6**) on the legibility of highway signs. Forbes' paper is principally methodological. Forbes and Holmes found that for dark letters on white grounds and for distances ranging from about 330 to 1550 feet, visual acuity increased somewhat with distance.

We wish in the present paper to supplement the work of Forbes and Holmes by presenting data on visual acuity for white letters on dark grounds for distances on the order of 30 feet, 300 feet and about two miles. The data in question were obtained as by-products in the course of work carried on over a period of over two years for Captain Luis de Florez, Director of the Special Devices Division, Bureau of Aeronautics, U. S. Navy, on a grant from the Commonwealth Fund (**2**).

Procedure

The stimulus object, in all cases, was a white E on a black background. The overall dimensions of the E were five times the width of the limbs. Observers indicated whether they saw the figure with the open end up, down, right, or left. The E itself was either white cardboard or canvas or wood painted flat white; spaces between the limbs of the figure were either black cardboard or canvas or wood painted flat black. In any case the contrast between figure and ground was high (about .90). Fig. 1 shows the single stimulus object, with overall size of $91\frac{7}{8}$ in., which was used in the experiments at distances between 1.16 and 2.83 miles. Fig. 2 is a photograph of a test board, similar to those used at fixed distances of 357, 343, 275 and 200 feet. A very similar photograph was used as the actual test board at the fixed distance of 25 feet. To provide data strictly comparable to those obtained with the large single E, one small-size replica of this E, with an overall size of $\frac{1}{4}$ in., was presented at distances of $16\frac{5}{8}$, $23\frac{7}{8}$, $30\frac{7}{8}$, and $40\frac{3}{4}$ feet, and another replica, with an overall size of $\frac{3}{16}$ in., was presented at distances of $12\frac{1}{2}$, $17\frac{7}{8}$, $23\frac{1}{8}$, and $30\frac{1}{2}$ feet. The various experiments are numbered from 1 to 8 in Table II. Table I gives the significant details of each of these experiments.

Visual acuity was defined as the reciprocal of the visual angle, in minutes, subtended by the limb of an E when the orientation of the E could just be discriminated. It was measured either

considers it clear that the K. phenomenon depends not on a law of form-perception, but upon a law of visual-sensation, and consequently holds the same to be true of the A.-F. phenomenon. Nevertheless, Jaensch considers both the A.-F. and K. phenomena to be centrally determined because in experiments he failed to find that micropsia decreased the differential limen for brightness (**13**, p. 146). Jaensch argues that when objects differ in apparent size, whether or not the retinal images differ, the central excitations differ in extension (**13**, p. 329). He believes that greater extension of excitation due to increase in apparent size results in lesser differentiation because the margins of the excitation then extend more into regions of diffuse, undifferentiated excitation.

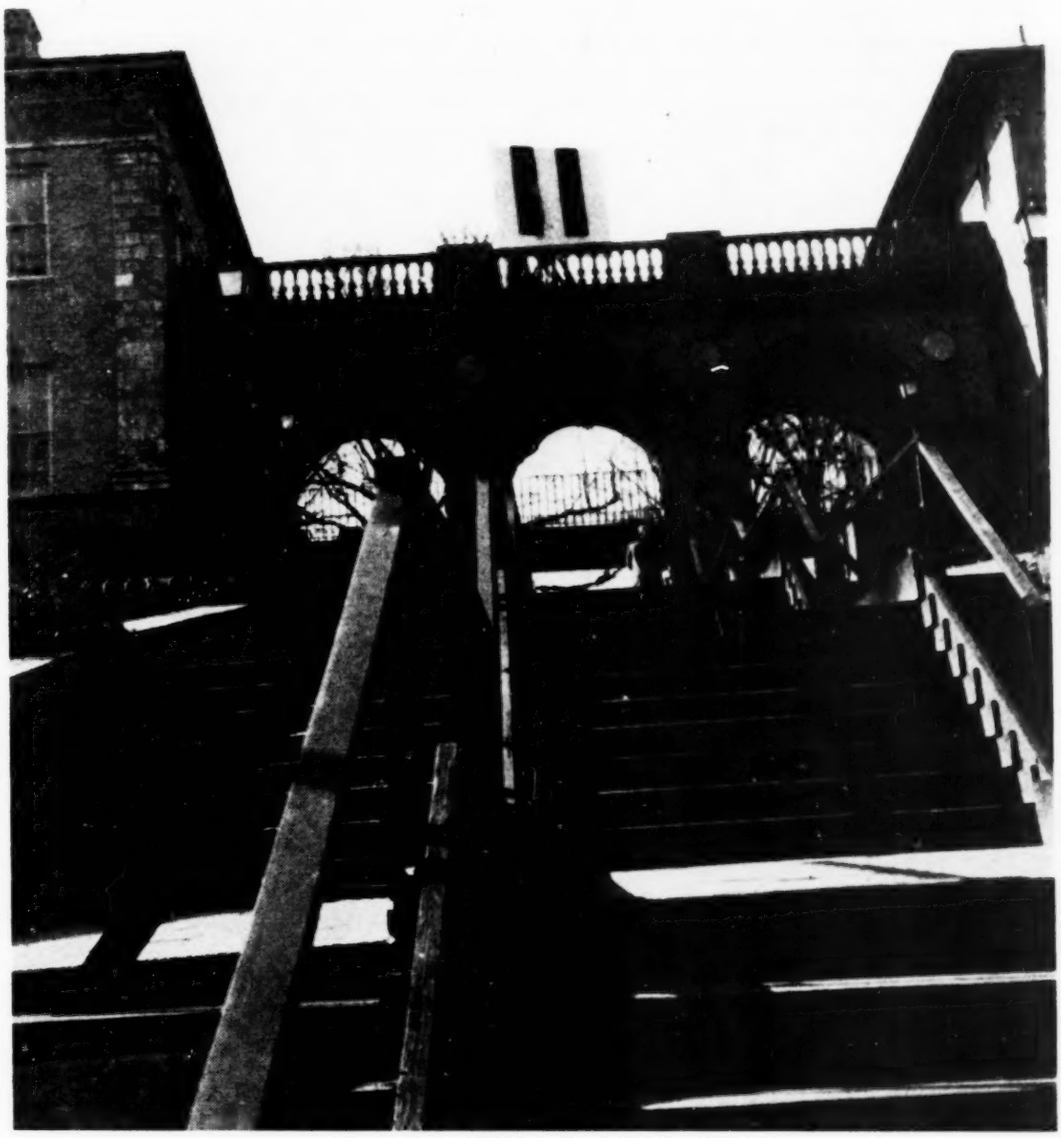

FIG. 1. Test-object used at distances between 1.16 and 2.83 miles.

by using the same test figure (Single E in Table I) and varying the distance between it and the observers, or by keeping the distance constant and using several stimuli (Multiple E's in Table I) of varying size. In both cases the technique was a modified psychophysical method of limits.

TABLE I

TYPE OF STIMULUS AND DISTANCE OF OBSERVATION IN EIGHT EXPERIMENTS ON VISUAL ACUITY

Experiment Number	Stimulus	Distance
1	Single E, $3\frac{1}{6}$ in. size	$12\frac{1}{2}$, $17\frac{7}{8}$, $23\frac{1}{8}$, $30\frac{1}{2}$ ft.
2	Single E, $\frac{1}{4}$ in. size	$16\frac{5}{8}$, $23\frac{7}{8}$, $30\frac{7}{8}$, $40\frac{3}{4}$ ft.
3	Photograph of multiple E's	25 feet
4	Multiple E's	200 feet
5	Multiple E's	275 feet
6	Multiple E's	343 feet
7	Multiple E's	357 feet
8	Single E, $91\frac{7}{8}$ in. size	1.16, 1.66, 2.14, 2.83 miles

In order to increase the reliability of this measure, the observations for a given visual angle in any experiment were repeated at least four times (usually 10 or 16 times) for a given stimulus. Since the angle subtended by the limbs of the E was known for any given judgment, it was possible to construct, for the various experiments, graphs representing as a continuous function the relation between number of correct judgments and visual acuity for a criterion of 100 percent correctness.

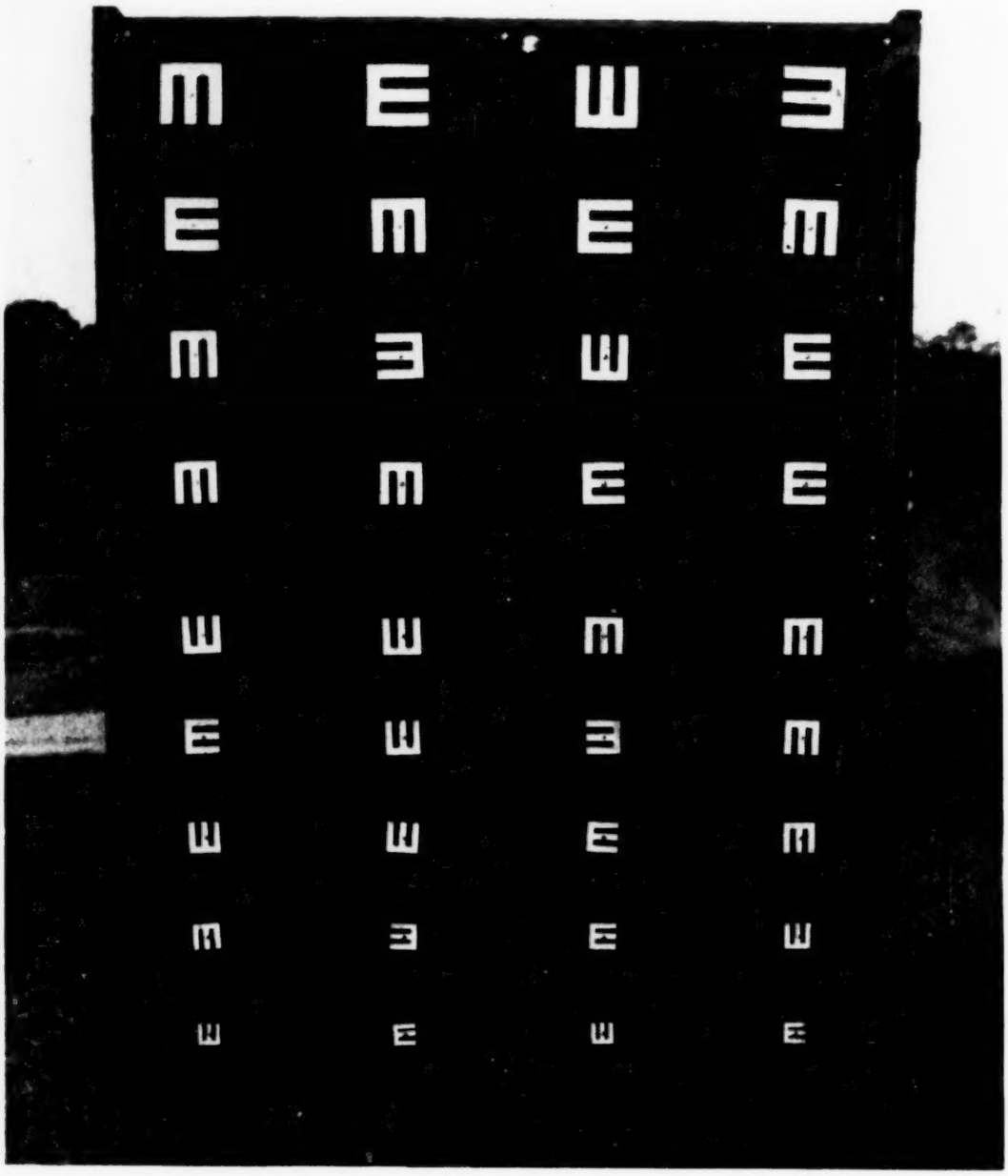

FIG. 2. Type of test board used at distances of 357, 343, 275, and 200 feet.

Scores, in the sense of total number of correct judgments, however distributed, were transformed into acuity values on the basis of such graphs.

RESULTS

Table II presents the totality of data in detail. The table includes a number of cases in which acuity was measured at only one

TABLE II

Visual Acuities at Eight Distances of Observation

Experiment No.	1.	2.		3.			4.			5.			6.	7.	8.			
Distance (feet)	12.5–30.5	16.7–40.8		25	25		200	200		275	275		343	357	6,125–14,942			
Brightness (ft. l.)	3511	3511		3635	3500		390	400		131	122		3000	4000	500	480	870	
Date	1/2/45	1/2/45		11/14/44	11/14/44		3/1/43	3/8/43		5/26/44	5/26/44		6/3/44	7/14/44	4/4/44	4/6/44	5/2/44	
*Name**			*Average*			*Average*			*Average*			*Average*						*Average*
Lee	2.05	2.05	2.05							2.21	2.08	2.15				2.00		2.00
Bedworth	1.81	1.96	1.89	1.58	1.63	1.61				1.75	1.83	1.79				1.77		1.77
Hunt	2.00	2.05	2.03													1.91		1.91
Phalen	2.15	2.28	2.22	1.48	1.48	1.48				2.08	1.92	2.00	1.94	1.94			2.10	2.10
Atkinson				1.80	2.40	2.10				2.50	2.08	2.29			2.48			2.48
Lybeck				1.15	1.13	1.14							1.57					
Walsh				1.70	1.70	1.70							2.23					
Drury				1.32	1.38	1.35								1.58				
Mead				2.15	2.40	2.28	2.13	2.37	2.25					2.10				
Beebe-Center				1.62	1.58	1.60	1.86	1.80	1.83									
Wagoner										1.83	1.75	1.79			1.96	1.91	2.14	2.00
Eklund										1.92	1.92	1.92		1.99			1.80	1.80
Raymond										1.83	1.92	1.88					2.14	2.14
Heilich													1.94	1.86	1.77			1.77
Simonds													1.78	1.75				
Nassi										2.50	1.92	2.24	1.82	1.92				
Hammond													1.68					
Ruggeri													1.70					
Dow													1.88					
Blake													1.56					
Johnson													1.34					
Tucker														1.17				
Fry														1.51				
Hayward														1.39				
McManus							1.75	1.95	1.85									
Proctor															2.35		2.35	2.35
Webster																1.22		1.22
Pettingill																	1.91	1.91

* All *Ss* were women undergraduates except Mead, Beebe-Center, and Wagoner.

distance. Although no use of such cases has been made in the figures and discussion below, they help to give the reader an idea of the range of individual differences involved among our *S*s, and also of the reliabilities of the measurements.

Fig. 3 presents, in terms of logarithmic coördinates, the data for all cases where measurements were available at two or more distances. It should be noted that the scale of the ordinates is five times as large as the scale of the abscissae. At first sight the figure seems to indicate an increase of visual acuity with distance for the range between 25 feet and 300 feet. We believe, however, that this

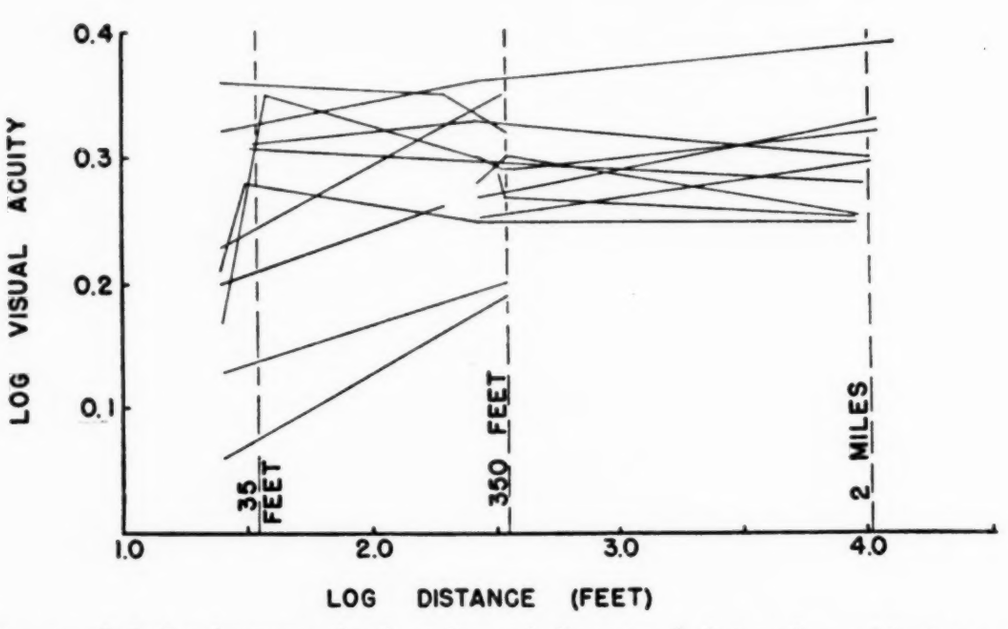

FIG. 3. Relation between visual acuity and distance of observation. All distances and all observers.

feature is an experimental artifact. As has already been pointed out, the stimulus at 25 feet was a photograph of the type of test board used at distances around 300 feet. It seems very likely that the relatively low visual acuities at 25 feet are simply due to decrease in contrast and sharpness of the stimulus object. What we think should be noted in Fig. 3 is the fact that the order of magnitude of changes in acuity is only about 0.1 of a logarithmic unit, or 26 percent, although the order of magnitude of the changes in distance is one logarithmic unit, or 900 percent, for the comparison at short and medium distances, and 2.5 logarithmic units, or 31,500 percent, for the comparison at short and long distances. Our interpretation of the figure, consequently, is that it shows marked stability of visual acuity for a range of 25 feet to around two miles.

This conclusion is strongly supported by Fig. 4, which represents that part of the data in Fig. 3 which we believe to be most reliable and significant. The figure shows for four observers acuities at

distances on the order of 30 feet (Experiments No. 1 and 2) and two miles (Experiment No. 8) measured by the same method (single *E*'s) and with strictly comparable stimuli. For all observers, visual acuity is slightly decreased at the larger distance. The largest change in acuity, however, involves .03 of a logarithmic unit, or 7 percent, while the change in distance involves 2.47 logarithmic units, or about 29,000 percent. Furthermore, the experiments were carried out in an industrial region, and a slight decrease in acuity with distance could consequently be expected from haze. Also, as shown in Table II, the brightness of the test object was less in observations at the two-mile distance. We believe that these data constitute strong evidence that visual acuity, expressed in angular

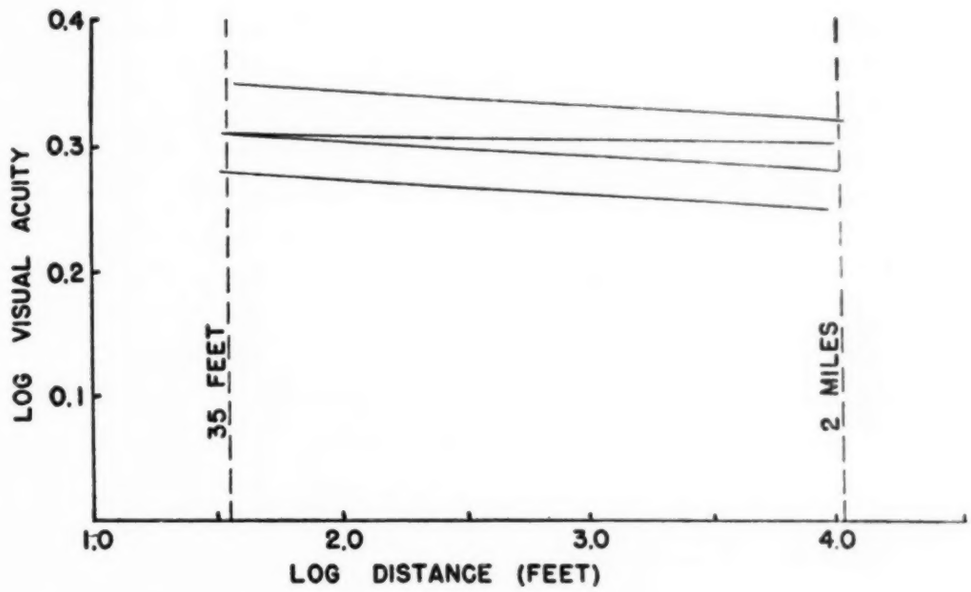

FIG. 4. Comparison of visual acuities at 30 feet and 2 miles for 4 observers.

terms, is independent of distance for the range from 30 feet to two miles.

This conclusion agrees entirely with the statement of Forbes and Holmes that "For practical purposes the relationship of letter height and width to legibility distance may be stated in terms of approximate straight lines" (**6**, p. 331). Forbes and Holmes add, however, that "actually, a curvilinear characteristic is noticeable between 6 and 10 in values," but do not venture on any interpretation. The magnitude of this effect is relatively slight: on the order of a 20 percent increase in acuity for a 370 percent increase in distance.

DISCUSSION

It was pointed out earlier in this paper that, according to a central theory of the variation of acuity with distance, such variation could

be expected to occur at large distances as well as small. Our findings, consequently, militate against such a central theory. If visual acuity varies with distance for distances of less than 30 feet, but not for greater distances, it would seem natural to turn to an explanation in terms of accommodation or some other ocular factor.

But just how strong is the evidence that visual acuity varies with distance for distances under 30 feet? As pointed out, Freeman believes the results in these experiments to depend upon method. Does dependence upon method by any chance mean dependence upon experimental artifacts? In the last twenty years a good deal of information has been obtained regarding the influence of configurational factors on border thresholds and visual acuity. Thus, glare sources have been found to cut down acuity to as little as $\frac{1}{10}$th under conditions of maximal effectiveness, and to operate at angular distances of as much as 25° (17, pp. 440 sq.). Again, the threshold for a border has been found to vary over a range of as much as one to two, as a function of the proximity and position of another border, for angular separations up to 4° (11). It seems clear, consequently, that no conclusion can safely be drawn from an experiment on the relation of acuity to distance unless the whole visual field, but more especially the proximate background, has been fully transposed, or unless the effect of such lack of transposition has been taken into account. Has this requirement been satisfied in the principal experiments on the relation of visual acuity to distance for short distances?

Let us first consider the experiments of Aubert, Jaensch, Jacobsson, v. Lempicka, Freeman and Berger which show a decrease of acuity with increase of distance between observer and object. Aubert's first experiment, in collaboration with Foerster, involved a decrease in the angular size of the background for part of his data (1, pp. 237 sq.). The observer looked through a tube allowing a visual field of at least 60°. The background was two 'feet' wide, which presumably means 60 cm. The distances from the observer varied from 10 to 100 cm., in steps on the order of 10 cm. At 10 cm. 60° gives a field 10.14 cm. wide. Thus, for this distance the background involved the whole 60° of the field of vision. This state of affairs continued for distances up to 50 cm. At 60 cm., however, 60° subtends 68 cm. Beginning with this distance, therefore, the background no longer was transposed, but rather, became smaller in terms of angular measurement. Aubert's second experiment involved the use of pairs of black squares 4, 8 and 20 mm. wide, separated by 4, 8 and 20 mm., respectively, on white paper. Aubert states specifically that the size of the white paper was transposed (1, p. 241). The size of the clamp holding the white paper, however, was not transposed.

It is impossible to judge, even from Aubert's original account of the experiment, what the organization was in the observer's field of vision. Thus, although it seems likely that in Aubert's second experiment the background was adequately transposed, this conclusion is by no means certain.

Jaensch does not specify completely the grey backgrounds which he used for black squares with a size ratio of 1 to 10. He does, however, state that the widths of the backgrounds were 15 and 22.5 cm., yielding a ratio of 1 to 1.5. An attempt to correct for this lack of transposition was made by placing paper similar to the background paper on a part of the wall back of the larger background (**13**, p. 27). As far as can be judged from the description, however, it seems fairly clear that Jaensch did not transpose his background adequately.

We come to Jacobsson. The test-objects were projected as dark objects on a light field on a screen (**14**, p. 25). The size of both the light field and the visible part of the screen were transposed. Jacobsson used three distances, approximately 2, 6 and 10 meters, and 2 kinds of stimuli, lines and letters. Comparing performance at 2 meters with performance at 10 meters, and performance at 6 meters with performance at 10 meters, the results indicate on the whole better acuity at the shorter distance. Comparing acuity at 2 meters with acuity at 6 meters, however, the results show better acuity for the longer distance for 5 of 9 observers in the case of lines, for 3 of 5 in the case of letters. The conclusion seems warranted that although Jacobsson's results were based on adequate transposition of the field, they are somewhat ambiguous with respect to effect of distance on acuity.

W. v. Lempicka used as test-objects grids of varying degrees of coarseness, viewed from various distances. Irrespective of coarseness and distance, however, the grids were always shown in a semicircular window 20 cm. in diameter. Thus the inner contour of the background was not transposed (**15**).

Freeman in various articles (**7**, **9**, **10**) cites data of his own indicating decrease of visual acuity with distance, but only in one article have we been able to find a description of procedure sufficiently detailed to indicate whether transposition of background occurred or not. In this one case (the article in *Psychol. Forsch.*), the experiment involved a principal background whose size was definitely not transposed.

As to Berger, in the case of experiments showing decrease of visual acuity with distance he definitely did not transpose the backgrounds. The figures whose size and separation were varied were always placed on a grey background 21 × 30 cm., irrespective of distance (**3**, **4**).

Let us now consider the data of Freeman and of Luckiesch and

Moss which show an increase in visual acuity with distance. Freeman used a very large white background, 3 × 1.8 meters, at distances of .3 and 3 meters. Within this background were two square windows, .2 × .2 meters and one .1 × .1 meters (**8**, **10**). The standard stimulus of a pair of black lines was on the edge of this window, the variable moved within it on a white background. Thus, although the lines were transposed, the windows were not. Luckiesch and Moss used two different test-objects, namely, a pair of white bars on a dark background and a dark and light grid (**16**). In both cases the test-objects were presented through a window in a large white surround. There was no transposition whatever except for the width and spacing of the bars or grid lines.

Finally, let us consider the data of Jaensch, Freeman and Berger which show no change of visual acuity with distance. In Jaensch's case (**13**), the first experiment yielding this result was one where black squares were pasted on grey cards, the size of which was transposed. The cards, however, were presented in front of a larger grey background, the size of which was apparently not transposed. No mention is made of the prominence of the outer contour of the card under these conditions. It is consequently impossible to judge whether transposition of the card was successful in maintaining the contour around the letter in the same angular position in the visual field. The second experiment by Jaensch which showed no notable change of visual acuity with distance was made with the same apparatus and technique as one of his experiments demonstrating the Aubert-Foerster phenomenon, except that instead of daylight illumination momentary artificial illumination was used. In daylight illumination transposition of the background had been but very partial. Whether this was so with artificial illumination is impossible to decide from the description of illumination given by Jaensch.

Freeman's procedure involved the presentation of luminous figures and letters in the dark (**8**). The background was thus simply darkness of the whole visual field, and transposition may be said to have been complete. Berger similarly presented luminous test objects, but under conditions of light adaptation (**4**). As far as can be judged from his account, the test-objects were presented against an untransposed background, as in his experiment with white and black test-objects, but the untransposed background appears to have been larger. It is hard to judge the conditions of illumination, but it appears that the contrast differences involved between background and surround were much less than between test-object and background. Incidentally, it should be mentioned that, although Berger interprets his results as showing essential constancy of visual acuity, they actually show a slight improvement of acuity with distance.

What conclusion can be drawn from these data concerning the relation of acuity to distance in the case of short distances? When the possibility of configurational effects is taken into account, none of the research purporting to show variation of acuity with distance is conclusive. Two of the experiments indicating constancy of acuity, on the other hand, were carried out under conditions minimizing or obliterating configurational differences at different distances, namely, with luminous sources. The suggestion seems fairly obvious that visual acuity does not vary with distance, even for short distances, except under the influence of configurational factors which, in the case of the relation being studied, constitute experimental artifacts. Other considerations, however, prevent us from drawing such a conclusion, even tentatively. In the first place, the experiments are not strictly comparable: Some (Aubert's first) cover distances from 10 cm. to one m., others (Berger's on luminous test-objects) distances from two m. to 25 m ; some (Aubert's) are on extreme peripheral vision, some (Jaensch's) on somewhat peripheral vision, some (Berger's) on central vision. In the second place, many of the experiments seem to have been carried out with very dubious control of such factors as illumination and improvement with practice. In the third place, and this is the most important consideration, it would be surprising if the complex mechanism of lenticular and pupillary adjustment—and of convergence, in binocular vision—maintained equal efficiency for all distances. Indeed, the existence of a 'near-point' proves that this mechanism breaks down markedly for very short distances; and the further existence of a slight myopia for low degrees of relative accommodation (**18**) suggests that this breakdown represents but the extreme of a gradual shift in accommodative efficiency from myopia at large distances through an optimum to hypermetropia at very short distances. In consequence, our conclusion in regard to the relation of acuity to distance for short distances is that there is need of a single careful experiment in which the function relating visual acuity to distance of test-object is studied both peripherally and centrally between one and 30 feet with adequate control of illumination and practice, with constant background and surround, and with supports for the test-objects subject to transposition in the same degree as are the test-objects themselves.

Summary and Conclusions

1. Eight experiments on visual acuity were carried out at observation distances varying from about 30 feet to about two miles. The results show that for practical purposes visual acuity, defined in angular terms, may be considered to remain constant over this range of distances. The observed discrepancies from constancy

were slight, relative to the distances involved, and seemed explicable in terms of changes in contrast and illumination.

2. Failure in this experiment to find any marked decrement in acuity at large distances is interpreted as support for a peripheral theory of the Aubert-Foerster phenomenon, insofar as such a phenomenon is demonstrable.

3. A survey of the literature centering around the Auber-Foerster phenomenon, however, shows that evidence on the relation of visual acuity to distance for short distances is inconclusive, and that there is need of a careful experiment in this field in which configurational factors are fully taken into account.

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PRIMARY AND SECONDARY SUGGESTIBILITY: AN EXPERIMENTAL AND STATISTICAL STUDY

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Introduction.—The concept of 'suggestibility' is of obvious importance to social as well as to abnormal psychology; it has long been used as an explanatory concept in both these spheres. Yet the evidence regarding the question as to whether or not we have the right to speak of 'suggestibility' at all, i.e., whether this term covers a simple, unitary mental trait, or whether it would be more accurate to speak of 'suggestibilities,' is very far from complete. The question of generality or specificity is still unsettled.

Averling and Hargreaves, in discussing their experimental findings, "consider that . . . the most probable explanation of [the] results is . . . the existence of a general factor of suggestibility, combined with group factors common to two or more tests" (**3**, p. 73). Similarly, Otis (**24**) believes in the existence of a general trait of 'ability to resist suggestion,' a belief also based on experimental evidence.

Brown (**9**), on the other hand, found little evidence of such generality in his pioneer studies, and Estabrooks (**11**) also had to report that the majority of correlations found by him were around zero. Allport (**1**) believes that suggestibility is a trait which may characterize a few people consistently, but that it is not otherwise a 'unitary' trait, while Britt (**8**) also is sceptical with regard to the existence of a general trait of this nature.

Many investigators believe in the existence of different types of suggestibility, and many schemes of such typological division have been worked out by Prideaux (**25**) and later writers. Thus, Hull distinguishes prestige and non-prestige suggestion, identifying the first-named with that found in his Body-Sway test, and the latter with the Binet Progressive Weights and Progressive Lines tests (**19**). Murphy *et al.* discuss three common pyschological principles underlying many 'suggestion' tests (**23**). Bird (**6**) speaks of direct and indirect suggestibility.

In a factorial study of eight tests of suggestibility, Eysenck (**12**) found no evidence of a general factor running through all the tests

[1] With the support of the Rockefeller Foundation. We are also indebted to the Superintendent of Mill Hill Emergency Hospital for permission to use the clinical material there.

and showed that two orthogonal factors were sufficient to account for all the correlations within the limits of the probable error. He called these two types of suggestibility primary and secondary; primary suggestibility was of the ideo-motor kind exemplified in such tests as the Body-Sway test, the Chevreul Pendulum test, and the Arm Levitation test, while secondary suggestibility was of the 'indirection' kind (**29**) exemplified by the Binet tests (**5**). It was further shown that the presence or absence of 'prestige' did not distinguish these two types of suggestibility. The possibility that further unrelated kinds of suggestibility might exist was left open.

Certain weaknesses in this experiment made it desirable to repeat it with some modifications. The number of different tests of suggestibility used was too small to give any definitive results; the tests of secondary suggestibility were too similar to make this concept very meaningful; no effort was made to include hypnosis among the variables, in spite of the acknowledged close relation between hypnosis and suggestibility.

An attempt was made to remedy these faults in the present study. The number of tests used was 12, including a fairly representative battery of tests of secondary suggestibility, and every *S* was rated on a scale of 'hypnotizability' similar to that developed by Davis and Husband (**10**). In addition to thus confirming and extending the previous findings, experiments were undertaken with a view to obtaining evidence regarding (1) the distribution of primary suggestibility, and (2) the nature of primary suggestibility. Superficially, scores on tests of primary suggestibility tend to be distributed in the form of a U-curve; an attempt was made here to investigate the actual distribution of the underlying trait. As regards the nature of primary suggestibility, it seemed worth while to test a theory relating primary suggestibility to ideo-motor action (**13**).

The experiment.—The experimental population used in this research consisted of 60 patients of the Mill Hill Emergency Hospital for Nervous Disorders, all of whom were in the Army at that time, and who were entirely unselected except for the fact that they were all of approximately the same intelligence. This was achieved by taking only patients whose scores on the Matrix test of intelligence put them roughly between IQ 90 and 110 (**26**); the test data may be regarded as comparatively reliable and valid, as previous work on the neurotic population of this hospital had shown their responses to be not very much different from comparative normal groups (**14, 15**). By this means, intelligence was practically ruled out as a factor in our experiment.

The tests used were almost all adapted from existing tests described in the literature. A novel test was included which had been developed at this hospital in an effort to study the effects of drugs on suggestibility (**17**). In order of giving, the tests were as follows:

(1) *Picture Report test.*—The patients were shown a painting of a church interior for 30 sec.; then the picture was removed and 14 questions regarding various details in it were asked. Five of these questions contained suggestions that certain details were contained in the picture which actually were not in the picture. The number of these suggestions accepted by the patient constitutes his score on this test.

(2) *Ink-Blot Suggestion test.*—One of the Rorschach ink blots was shown to the patient, and he was told that some people thought it looked like [here followed two common responses to the

blot]. Then he was given four quite inapplicable responses and asked if he could see something resembling these in the ink blot. The number of suggestions accepted constituted his score.

(3) *Chevreul Pendulum test.*—The patient was shown a small pendulum made of a bob suspended by a thread. He was told that if he held the pendulum over a ruler, and looked fixedly at the bob he would quickly notice that the bob started to swing along the ruler, even though he himself remained quite passive, and was careful not to produce a movement deliberately. *E* demonstrated how the pendulum started swinging, then handed it to the patient with the instruction to hold it steady and look fixedly at the bob. Continuous strong suggestion was then given to the effect that the bob was beginning to swing, that the swing was increasing, etc. The actual swing of the pendulum in inches constituted the score on this test.

(4) *Odour Suggestion test.*—The patient was told that his sense of smell was to be tested. Six dark green bottles were placed before him, labelled, in order: Pineapple, Banana, Vanilla, Rose, Jasmine, and Coffee. He was told that the cork would be removed from each bottle in turn, and that the bottle would then be brought slowly up to his nose from a distance. He was to report as soon as he could detect the smell. The cork was then removed from the first bottle (pineapple essence) while it was some two feet from the patient's nose, this distance then being decreased until either the patient reported that he could detect the smell, or the bottle was in contact with his nose. The procedure was repeated with each of the other bottles in turn, the patient being told each time what odour to expect. The last three bottles contained only water, and each claim to detect an odour from these bottles was scored 1 point. The first three odours were selected in such a way that they decreased in strength, pineapple being the strongest, vanilla the weakest.

(5) *Progressive Weights, Impersonal.*—Twelve boxes identical in appearance were placed in front of the patient, who was told that they all differed in weight, and he was to compare box 1 with box 2, 2 with 3, 3 with 4, and so on, saying each time which of the two was heavier. The first five boxes increased in weight by identical amounts, the next seven boxes were identical in weight with the fifth box. The score was the number of identical boxes called 'heavier' minus the number of identical boxes called 'lighter.'

(6) *Progressive Weights, Personal.*—For this test, the raw data from test (5) were used, but the scoring was such that the number of times the identical boxes were called 'heavier' and the number of times they were called 'lighter' were added. The reason for these different methods of scoring has been given elsewhere (12); roughly, it consists in counting in test (6) the effect of the personal suggestion that the weights were all going to be different, and in test (5) the effect of the impersonal suggestion emanating from the arrangement of the material that the weights would continue to get heavier.

(7) *Heat Illusion test.*—A small heating element was applied to the forehead of the patient, the element being connected through a variable resistance, an invisible switch, and a transformer to the electric mains. The patient was shown how the element became hot as he turned a calibrated knob connected with the variable resistance. He was asked to turn the knob slowly until he could just detect the first sign of heat in the element, when he was to remove the element from his forehead immediately and call out the reading on the dial. The patient was then asked to repeat the procedure, and on this occasion the secret switch was silently opened, so that no current passed through the element. As the dial reading approached that at which heat had been reported previously, the patient's attention was drawn to this fact, and he was told: "Be on the alert now, you should soon feel the heat." Those who reported feeling the heat when the switch was open were scored as suggestible.

(8) *Body-Sway test.*—This test consists in the measurement of the effect of verbal suggestions that the patient is falling forward, continued for 2½ min., on the posture of the patient who is trying to stand still and relaxed, with his eyes closed. The amount of sway induced is measured in inches through inspection of a pointer on a scale which is activated through a thread fastened to the patient's clothing. The score on this test is the maximum amount of sway induced, in inches; complete falls are arbitrarily scored as 12 inches.

(9) *Press and* (10) *Release test.*—This test measures the effect of verbal suggestion that the patient is either releasing his hold on, or is grasping more firmly, a rubber ball. This ball is connected through rubber tubes with a particularly large tambour, which in turn activates a lever which writes on a kymograph, thus making a complete record of the pressure exerted by the patient on the rubber ball. The patient is fully aware that he is expected to keep his pressure steady, just as in the preceding test he knows that he is expected to keep standing still. In the Press test the patient starts by holding the ball so that he exerts just a small amount of pressure

(in order to enable him to give a negative reaction to the suggestion); in the Release test the patient starts by squeezing the ball hard so that he should be able to relax to a considerable extent. (Here also a negative reaction is possible.) Scores are calculated in terms of maximum excursion of the lever.

(11) *Hypnosis.*—An attempt was made to induce hypnosis in the patients by a method combining fixation of a bright object, a constant low sound, and verbal suggestion. A variety of suggestions regarding eye closure, tiredness, rigidity, anaesthesia, catalepsy, positive and negative hallucinations, etc., were given; the complete list of these suggestions will be found in the next section, Table I. These suggestions under hypnosis were given in the same order for each patient; the sequence used is indicated in Table I. Scoring of this test is explained in the next section.

(12) *Post-hypnotic suggestion.*—Three post-hypnotic suggestions were given. (1) On repetition of the Body-Sway test there would be an increase in sway; (2) there would be a definite negative error in a length-comparison test which had been given previous to the hypnosis; (3) the patient would shake and open a box on coming out of the hypnosis. The scoring of this test also is given in the next section.

In addition to these 12 tests, two tests of the effects of autosuggestion were carried out, in an effort to settle certain theoretical points discussed in a later section. In these, the patient was put in the same position as at the beginning of the Body-Sway test, but instead of suggesting to him that he was falling, he was simply requested to imagine that he was falling, and left to himself after that. This autosuggestive test was done for 30 sec. each time.

A further experiment was carried out on a different population from that investigated in the main part of the present research. A group of 70 patients were given the Body-Sway test under conditions of heterosuggestion by means of a gramophone record for 30 sec., their sway in response to the record being noted. Then the experiment was repeated, the time during which the record was being played being increased to 2½ min. Again, maximum response to the suggestion was noted for each patient. This experiment was carried out in order to obtain certain data necessary in our discussion of the distribution of primary suggestibility. It was supplemented by another experiment in which 100 men were given the Body-Sway test in full, and had it repeated immediately on them, again lasting the full term of 2½ min. This was done in order to obtain data on practice effects.

Results.—In Table I are reported the results of the hypnotic experiment. The response to each one of the suggestions made was rated on a three-point scale: if the response was complete, the *S* scored two points for this particular part of the test; if the response was definitely noticeable, but fell short of completeness, it was scored one; if there was no observable response at all, the *S* received a score of zero on this particular item. Total number of points thus gained by the 53 *S*s who completed this test are given after each item; thus, the fact that the item 'Eyes closing' received 45 points shows that on the average this suggestion was followed much more frequently and whole-heartedly than the suggestion 'Complete catalepsy,' which received only 21 points.

In order to get a total score for each person, the items making up the scale were weighted. The weights are roughly inversely proportional to the number of points scored by each item; thus, the items to which a good many people were suggestible are weighted less than those to which only a few proved suggestible. The weights are also given in Table I.

A special method of scoring was used for the item 'amnesia.' If 10 or fewer items of the hypnotic test were recalled or recognised

TABLE I

RESULTS OF THE HYPNOTIC EXPERIMENT

Items Suggested *	Points	Weights
1. Eyes tired	76	1
4. Complete relaxation	76	1
6. Feels incapable of activity	65	1
8. Arm falls irresistibly	63	1
2. Eyelids heavy	61	1
9. Impossible to raise arm	59	1
7. Feels miles away	54	1
5. Feels pleasant warmth	53	1
12. Glove anaesthesia	46	2
3. Eyes closing	45	2
10. Both arms stiff and rigid	36	2
19. Impossible to raise arm (eyes open)	24	3
17. Illusion of bell-ringing	22	3
11. Complete catalepsy	21	3
18. Cannot hear buzzer	19	4
16. Illusion of leg movement	18	4
13. Increase of Body Sway (post-hypnotic)	18	4
14. Line length suggestibility (post-hypnotic)	16	4
15. Shake and open box (post-hypnotic)	13	4
20. Illusion of electric bulb lighting up	12	4
21. Amnesia (spontaneous)	15	4

* The suggestions are numbered in the order in which they were given. Post-hypnotic suggestions were given as indicated, and the effect tested after the whole scale had been given to the patient. The amnesia observed was spontaneous, as no suggestion to the effect that the patient would forget anything was given.

during interrogation following the test, the *S* got two points; if from 11 to 20 items were recalled or recognized, he got one point; if more than 20 items were recalled or recognized, he got zero points.

Separate scores were obtained for hypnosis and for post-hypnotic suggestibility, the latter being made up of items number 13, 14 and 15. The distribution of scores on the Hypnosis test is shown in Fig. 1; scores on this test range from 0 to 76. Means and standard deviations on this test are reported in Table II.

Also reported in that table are the means and SD's of the other tests used. The histograms showing frequency of scores on these tests are given in Figs. 1 and 2. It will be seen that roughly speaking the distribution of scores on the tests of primary suggestibility tends towards a U- or a J-shaped curve, while scores on the tests of secondary suggestibility tend towards a normal curve of distribution.

Tetrachoric correlations were worked out between the 12 tests used, and are given in Table III. We preferred the use of tetrachoric coefficients to that of the product-moment formula because of the very far from normal distribution of the scores on many of the tests. Further justification for the use of this formula (which strictly speaking applies only when the underlying variable is normally distributed) is given in a later section, where the distribution of the underlying trait is discussed.

The average intercorrelation of the six tests of primary suggestibility (Nos. 3, 8, 9, 10, 11, 12) is + 0.50; the average intercorrelation

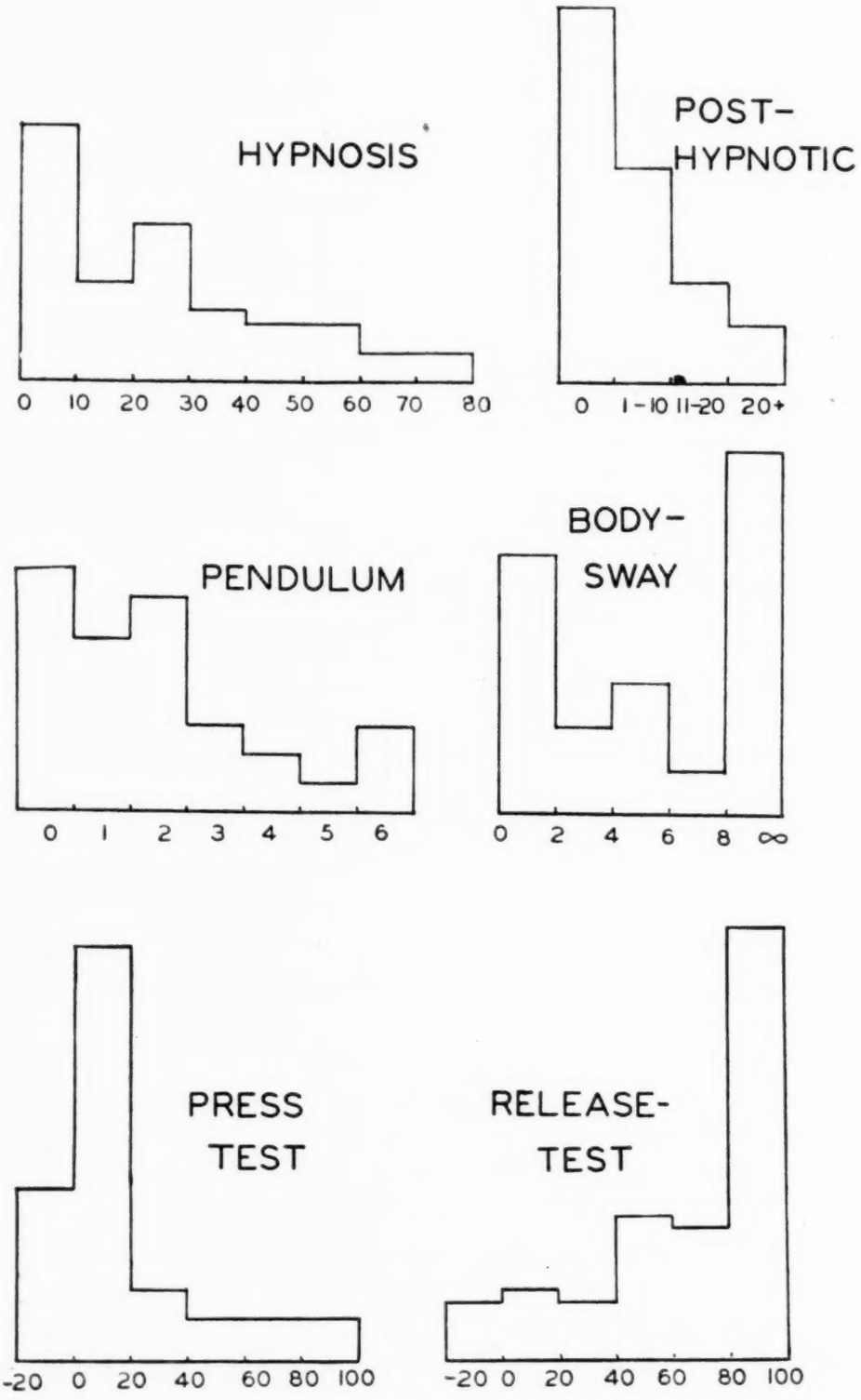

FIG. 1. Distribution of scores on six tests of Primary Suggestibility

of the six tests of secondary suggestibility (Nos. 1, 2, 4, 5, 6, 7) is + 0.15. The average intercorrelation of the tests of primary suggestibility with those of secondary suggestibility is + 0.02. As we

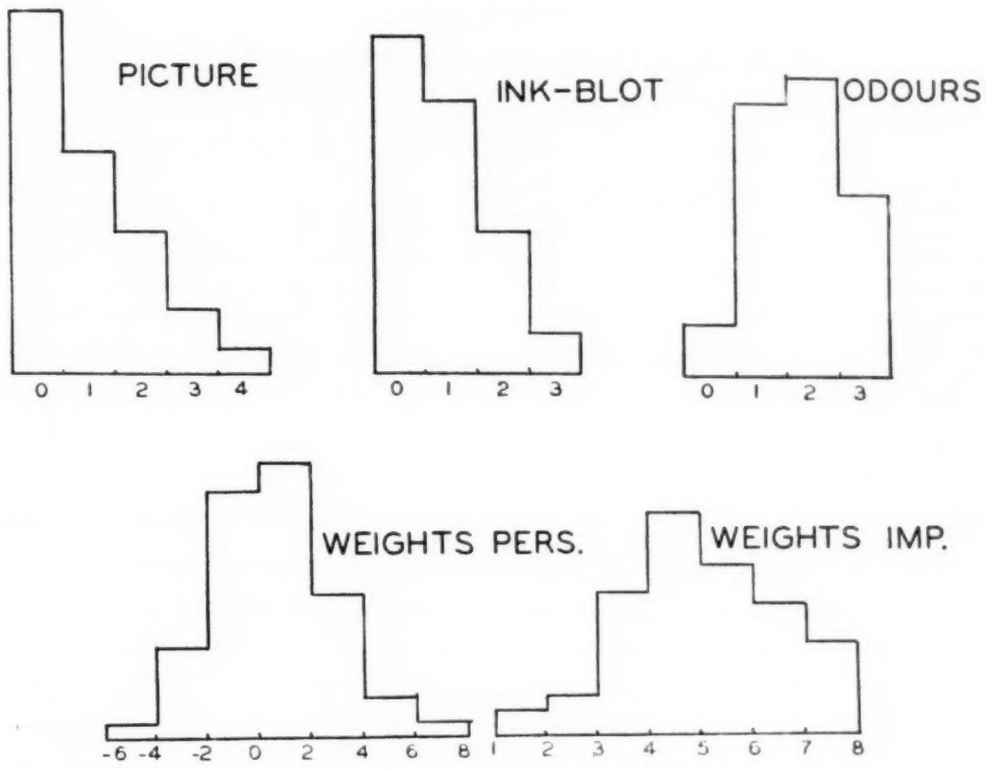

FIG. 2. Distribution of scores on five tests of Secondary Suggestibility

are therefore clearly dealing with two entirely unrelated factors, there would appear to be little point in factor-analysing the table as

TABLE II

MEANS, STANDARD DEVIATIONS, AND FACTOR SATURATIONS OF THE TESTS

	Mean	S.D.	Factor Saturations: Primary Suggestibility
Hypnosis	23.0	20.7	.89
Post-hypnotic	1.6	1.9	.77
Pendulum	2.0	1.9	.64
Body Sway	6.5	4.7	.92
Press Test	12.2	23.9	.38
Release Test	50.7	50.5	.73
			Secondary Suggestibility
Report	1.00	1.11	.27
Ink Blot	.83	.84	.71
Odour	1.60	1.20	.62
Weights, Imp.	.79	2.39	.06
Weights, Pers.	4.49	1.41	.43
Heat Illusion	−.21	—	.25

a whole; results would merely be vitiated by the chance-correlation of the tests of one group with those of the other.[2]

[2] A factor analysis of the whole table was actually carried out; the main findings were very similar to those reported in an earlier paper (12). They are not given or discussed because the practice of analysing a table of this kind as a whole is not beyond criticism.

TABLE III

TETRACHORIC CORRELATIONS AMONG THE 12 TESTS

Test	1	2	3	4	5	6	7	8	9	10	11	12
1. Hypnosis	—	.72	.57	.73	.24	.64	.51	−.10	.13	.12	−.17	−.10
2. Post-hypnotic		—	.54	.57	.24	.54	.29	.06	−.04	−.17	.09	.30
3. Pendulum			—	.75	.00	.38	−.07	−.07	.22	.06	.24	−.04
4. Body Sway				—	.47	.45	−.16	−.21	−.12	.18	.05	−.25
5. Press Test					—	.47	.10	.18	−.22	−.19	.27	−.16
6. Release Test						—	.02	.10	−.18	−.03	−.22	−.23
7. Heat Illusion							—	.31	.22	.18	.05	−.25
8. Picture								—	.31	.07	−.23	.10
9. Ink Blot									—	.24	−.02	.48
10. Odours										—	.24	.38
11. Weights, Imp.											—	.11
12. Weights, Pers.												—

Consequently, the two groups of tests were analysed separately, and the saturations with the one general factor found in each of the two groups of tests are given in Table II. It will be seen that the primary suggestibility factor accounts for 55 percent of the variance, the Body-Sway test having a saturation of .92 for this factor, and Hypnosis having a saturation of .89. It will also be seen that the secondary suggestibility factor accounts for 20 percent of the variance, the Ink-Blot Suggestion test having a saturation of .71 for this factor, and the Odours test having a saturation of .62.

Multiple correlations were calculated in an effort to determine the possibility of predicting success in hypnotizing a patient from a knowledge of the scores on the various tests of suggestibility used. In particular, the Body-Sway, the Pendulum, the Release and the Heat Illusion test were studied in this connection. A special difficulty is raised by the fact that the Heat Illusion test shows a tendency to correlate with the tests of secondary suggestibility, rather than with those of primary suggestibility, and that its only high positive correlation with any test of primary suggestibility is with hypnosis. This fact is discussed in its theoretical bearings in a later section; here we need only note that precisely because of the lack of correlation of this test with the other tests of primary suggestibility does it contribute a considerable share to the multiple correlations with hypnosis.

Various combinations of the tests mentioned, and their multiple correlations with hypnosis, are given below in Table IV. It will be noticed that Body-Sway and Heat Illusion together correlate + 0.96 with Hypnosis; thus, we may say that the factors active in these two tests account for over 90 percent of the factors determining a person's hypnotizability.

The results of our study of the whole battery of tests used may be supplemented by a report of the results of the two supplementary

TABLE IV

MULTIPLE CORRELATIONS OF CERTAIN TESTS WITH HYPNOSIS

Multiple correlation of hypnosis with: Pendulum and Body Sway	.73
Multiple correlation of hypnosis with: Pendulum and Release	.73
Multiple correlation of hypnosis with: Pendulum and Heat Ill.	.79
Multiple correlation of hypnosis with: Body Sway and Release	.80
Multiple correlation of hypnosis with: Body Sway and Heat Ill.	.96
Multiple correlation of hypnosis with: Release and Heat Ill.	.81

experiments undertaken in order to clarify certain points regarding the Body-Sway. First, the average amount of sway on the Body-Sway test, under conditions of heterosuggestion, and under conditions or autosuggestion (twice repeated), is given in Table V, together with the SD's of the three means.[3] Also given in that table are the correlations between the three tests.

TABLE V

MEANS OF BODY-SWAY MEASURES, STANDARD ERRORS OF THESE MEANS AND INTERCORRELATIONS AMONG THE THREE MEASURES

	Mean	σ_{Mean}	
(1) Heterosuggestion	6.8	4.8	$r_{12} = .92$
(2) Autosuggestion I	6.2	4.9	$r_{23} = .96$
(3) Autosuggestion II	6.4	4.9	$r_{13} = .93$

It will be seen that the means and the SD's are closely similar, and that the three tests correlate so highly that they must be regarded as identical. (The reliability of the Body-Sway test is + 0.91, as established in an earlier investigation, while the reliability of the Autosuggestion test, as shown above, is + 0.96. Consequently, if the two tests measured exactly the same trait, they should correlate to the extent of + 0.93, which corresponds precisely to the observed value.)

In the second experiment, in which 70 Ss were given the Body-Sway test (heterosuggestion by means of a gramophone record) for 30 sec., the test then being repeated for $2\frac{1}{2}$ min., the average amount of sway in inches increased from 4 to 5 inches, and 35 of the patients swayed over an inch *more* the second time, while only one swayed *less* the second time. Twenty fell outright during the 30-sec. run, while an additional six fell when the run was extended to $2\frac{1}{2}$ min. Thirty-nine men swayed less than two inches during the short run; only 15 swayed less than two inches during the long run. There is little doubt that this increase in the effectiveness of the record is due to the fact that it was played for a longer period of time; no practice effects were observed on simply repeating the test in identical form on another sample of 100 men.

Discussion.—The experimental results reported in the preceding

[3] These values are not quite identical with those in Table II because a few patients did not do the Autosuggestion test, and are not therefore included in Table V.

section require discussion on three points. These are (1) the question of the generality of the trait of suggestibility, (2) the question of the distribution of the trait of suggestibility, and (3) the question of the nature of suggestibility.

(1) The generality of the trait of suggestibility. The correlations reported among the 12 tests used in this study, as well as the results of the factor analyses carried out, seem to show clearly that we are dealing in these tests with two essentially unrelated traits, which in a previous study were called primary and secondary suggestibility (**12**). As in that study, primary suggestibility seemed to be of an ideo-motor kind, while secondary suggestibility seemed rather to depend on suggestion by indirection. Possibly a better name for this secondary kind of suggestibility might be 'gullibility.'

It is apparent from the intercorrelations among the various tests that primary suggestibility is a much more definite and marked trait; intercorrelations among tests of secondary suggestibility are all rather small, averaging only .15, as contrasted with an average of .50 for the other group. In addition, as was found in the previous study retest, reliabilities tend to be a good deal higher for tests of primary suggestibility than for tests of secondary suggestibility, the former averaging about .90 in our experience, the latter usually ranging between .30 and .50. Possibly this low reliability of tests of secondary suggestibility accounts for the low intercorrelations between these tests.

One test in our battery seems to create certain difficulties in respect to this differentiation into primary and secondary suggestibility, viz., the Heat Illusion test. This test has a positive correlation of .51 with hypnosis, but correlates negatively or only very slightly positively with Body-Sway and the rest of the tests of primary suggestibility not involving hypnosis. For this reason we have put it with the tests of secondary suggestibility, but clearly this decision is to some extent arbitrary. Our data do not really permit us to say whether this test belongs with the tests of primary or with the tests of secondary suggestibility, and only further research will enable us to answer this question satisfactorily. Possibly both types of suggestibility are involved in this test.

The question as to the proper alignment of this test is of importance because of its correlation with hypnosis. In attempting to forecast a person's hypnotizability from the results of psychological tests, this test contributes a good deal to the total forecasting ability of a battery precisely because of its low correlation with the other tests. As the multiple correlation coefficients given in Table IV show, the addition of the Heat Illusion test to the Body-Sway test raises the forecasting efficiency from 52 percent to 93 percent.

One further point deserves mention. In a previous paper, attention was drawn to the fact that there seemed to be a division within the factor of primary suggestibility between *active* and *passive* suggestibility. This appeared quite clearly in the Arm Levitation test, which was given in two forms: (1) the suggestion was that the arm was getting lighter, and was rising; (2) the suggestion was that the arm was getting heavier, and was falling. It was thought that these two forms of the test were almost identical, just as for instance the two forms of the Body-Sway test used in that experiment [(1) with suggestion to fall forward, (2) with suggestion to fall backward] were almost identical, as indicated by the correlation of .91. The correlation between the two forms of the Arm Levitation test, however, was only .35, thus indicating a much less close correspondence between the two tests than had been anticipated.

From observation it appeared that the *Ss* tested fell into two classes: (1) the alert, active type, who executed every suggestion rapidly, and (2) the passive, inactive type, who tried to achieve a state of complete restfulness, and who was much keener on carrying out suggestions which led to such a state (e.g., lowering his arm), than on carrying out suggestions which had the opposite effect (e.g., raising his arm). This distinction is one which can also be observed in hypnotic *Ss*.

In the present experiment, the Press and the Release tests, respectively, seem to divide the *Ss* into the same two classes, the active responding best to the Press, the passive to the Release test. In both the previous experiment and in the present one also, the test of *passive* reaction has a higher correlation with the other tests, and therefore has a higher factor saturation, than has the test of *active* reaction; the respective values are .37 and .55 in the previous, and .38 and .73 in the present experiment. This suggests that primary suggestibility as measured by our tests is of the passive rather than of the active kind. Similarly, hypnosis seems to go with the passive rather than with the active type of response, the respective correlations with the Release and the Press tests being .64 and .24. These findings suggest that further studies of the active-passive dichotomy may be fruitful in throwing light on the nature of suggestibility.

(2) The distribution of the trait of suggestibility. It has usually been found by investigators (**19**, **11**, **3**, **12**) that primary suggestibility scores are distributed in the form of a U-curve, rather than of a normal curve. The present study is no exception to this rule, as can be seen from the curves of distribution given in Fig. 1. The question arises whether this mode of distribution represents accurately the distribution of the underlying trait, or whether it is merely a statistical artefact. An attempt will be made here to show that the

latter alternative is the correct one, and that by reference to two psychophysical concepts, viz., that of *threshold* and of the *Weber-Fechner law*, the underlying distribution can be shown to be a normal one.

Let us assume that the trait 'suggestibility' is distributed in the form of a normal curve, as shown in Fig. 3*A*. Let us also assume

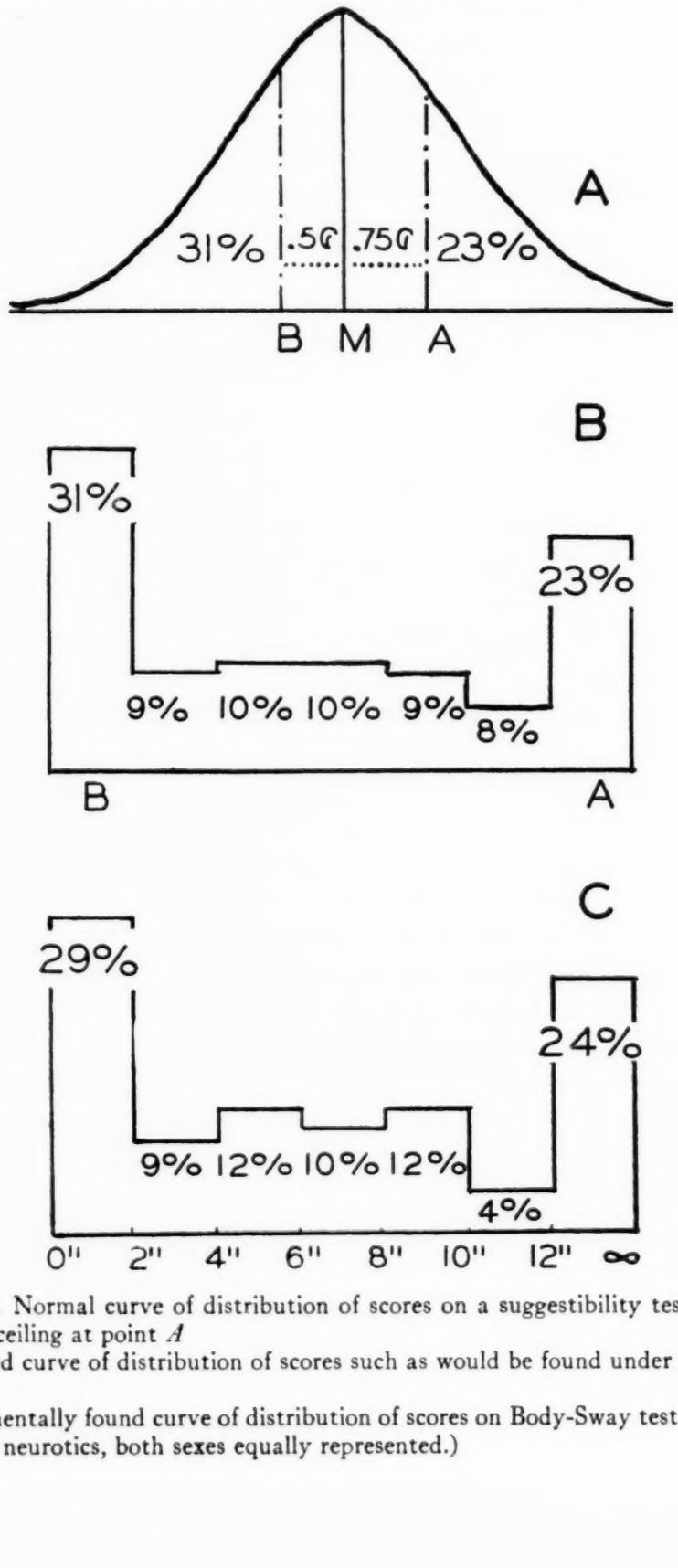

FIG. 3. *A*. Normal curve of distribution of scores on a suggestibility test, with threshold at point *B* and ceiling at point *A*

B. U-shaped curve of distribution of scores such as would be found under conditions shown in (*A*)

C. Experimentally found curve of distribution of scores on Body-Sway test. (Experimental population: 300 neurotics, both sexes equally represented.)

that the test has a threshold at point *B*, .50 sigma units below the mean, and a ceiling at *A*, .75 sigma units above the mean. In other words the test is discriminative only between *A* and *B;* values beyond those two points are either complete successes or complete failures. If this were so, the 31 percent of values below *B* would all come to lie at *B* in the actual distribution of the test scores, and the 23 percent of values above *A* would come to lie at *A*. This would effectively convert a normal curve into a U-shaped curve, with two modes at *A* and *B* respectively (*cf.* Fig. 3*B*).

Does this argument apply to the tests of primary suggestibility with which we are dealing? Let us restrict ourselves to the Body-Sway test. It is clear that there is an 'upper limit' beyond which the testee cannot go, viz., a complete fall. Similarly, there is a lower limit below which the testee cannot go, viz., no sway at all.[4] This much is self-evident; the question that remains is the following: Do the *S*s belonging to either of the two modal categories, i.e., those who fall outright, and those who do not sway at all, form homogeneous groups, i.e., groups containing *S*s all of exactly the same degree of suggestibility, or do they form heterogeneous groups, i.e., groups containing *S*s of different degrees of suggestibility? If the former hypothesis were true, it would follow that the U-shaped curve of distribution is the true mirror of the underlying distribution; if the latter hypothesis were true, it would follow that the U-shaped curve would not give a true picture of the underlying distribution. We can prove that the latter hypothesis is true in two ways.

The first method open to us is the one described in the experimental section, in which the same group of patients were given the Body-Sway test, first for 30 sec., then for $2\frac{1}{2}$ min. If the two groups were really homogeneous, then all those who had not swayed at all on the first test should have behaved similarly on the second test, and all those who fell outright on the second test should have behaved similarly on the first test. The figures quoted in the previous section clearly show that this was not so. On the second test, six *S*s fell who had not fallen on the first test; they were clearly less suggestible than the 20 who had fallen on the first test, yet because of the 'upper limit' the second test failed to distinguish them from the 20 more suggestible ones. Similarly, at the other end, of the 39 who had swayed less than two inches during the short run, over half increased their sway to above two inches during the long run, thus showing that this group of 'non-swayers' also was not homogeneous.

By thus extending the run to several minutes, by increasing the volume of sound coming from the gramophone, by injecting sodium

[4] In practice, a sway less than two inches is counted as 'no sway at all,' because many *S*s sway up to two inches even without any suggestion. This fact does not invalidate the argument, however.

amytal intravenuously, and by various other changes it is possible, and in fact quite easy, to show that neither the 'non-sway' nor the 'fall outright' groups are in any way homogeneous, but that they are made to appear so because of the limitations of the test, which has a high threshold and a low ceiling, so to speak.

The second method by means of which we can prove the same point is by showing that the *S*s who fall into either of our two groups in question do not react similarly to other tests of primary suggestibility. If all the *S*s who fall outright, for instance, were equally suggestible, then one would expect them to react in an identical manner to other tests of suggestibility. This, however, is emphatically not the case. This can be shown best, perhaps, by adding the weighted scores of the *S*s on all the six tests of primary suggestibility used, thus deriving a composite score for each *S;* the curve of distribution of these composite scores is shown in Fig. 4*A*. (The formula used for weighing the scores was

$$w_u = \frac{r_{ug}}{(1 - r_{ug}^2)\sigma},$$

in which w_u = the weight ascribed to test *U;* r_{ug} = the factor saturation of test *U* with the general factor of primary suggestibility; and σ the SD of the distribution of scores on test *U*.) The fact that this curve of distribution has lost the U-shaped appearance characteristic of the individual curves which go to make it up proves that neither the non-suggestible nor the very-suggestible groups in any of the individual tests are really homogeneous.

The question arises whether the resulting curve of distribution gives an accurate picture of the distribution of the underlying trait. We do not believe that it does. We have arbitrarily taken each unit along our base-line as equivalent to each other similar unit; we have taken the difference in body-sway between 0 and 3 inches as equal to the difference between 6 and 9 inches. There is no ground for supposing that such equality actually obtains; quite on the contrary, experiments conducted with neurotic and with normal *S*s have shown that while the difference between 0 and 3 inches is of very great significance, that between 6 and 9 inches is of very little importance (**26**). It seems reasonable to assume that additional increments of sway obey the law of diminishing returns, and consequently that some form of the Weber function seems appropriate here.

Accordingly, scores were converted into logarithmic values, and the resulting curve of distribution is shown in Fig. 4*B*. This curve approaches normality, though it is seriously skewed towards the left. This is understandable. It has been shown elsewhere (**16**)

that neuroticism is highly correlated with suggestibility; as our experimental population consisted entirely of neurotics, it is only natural that the curve of distribution of scores on suggestibility should be skewed towards the 'less-suggestible' end.

We believe we have shown in this discussion that it is unlikely that suggestibility is distributed in the form of a U-curve; we do not

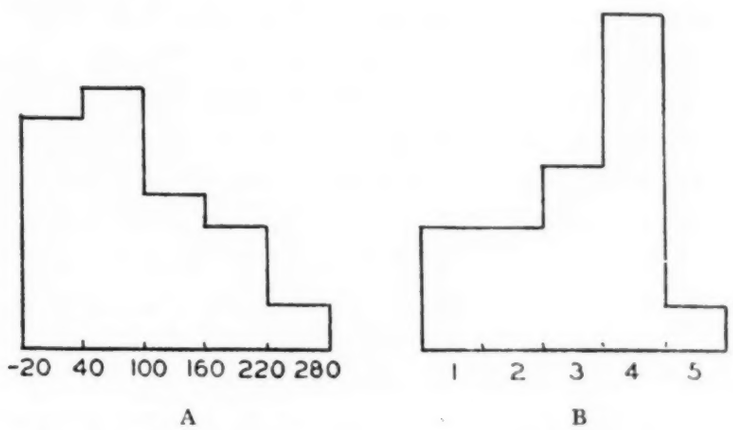

FIG. 4. Weighted combined scores on six tests of Primary Suggestibility. (*A*) Simple scores, (*B*) Log. scores.

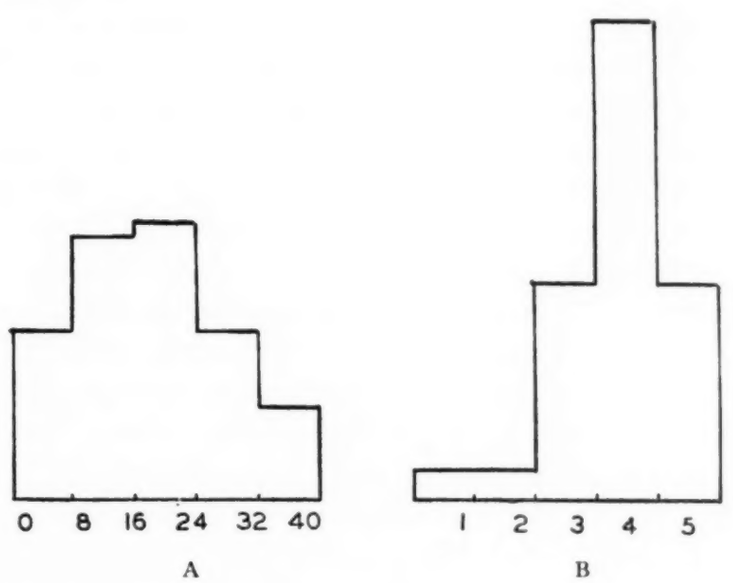

FIG. 5. Weighted combined scores on six tests of Secondary Suggestibility. (*A*) Simple scores, (*B*) Log. scores.

believe that our data are sufficient to prove that it is normally distributed, although the argument would certainly seem to tend that way. Proof that the Weber-Fechner law applies to suggestibility scores would certainly be required before accepting our suggestion; we do not claim to have done more than to show that it is not unreasonable to apply this law to our data.

For the sake of interest, we have also calculated the weighted

total scores of the six tests of secondary suggestibility; the distribution is shown in Fig. 5*A*. Here also the Weber-Fechner law may be applied, and the results of transforming the scores into logarithmic values are shown in Fig. 5*B*. This distribution also is unimodal, and approaches the normal.

(3) The nature of primary suggestibility. It has often been suggested that primary suggestibility and hypnosis are explicable in terms of the ideo-motor theory of action (**19, 13**). This theory has been combined with the view that in primary suggestibility we are dealing with two factors, aptitude and attitude, in such a way that the result of the Body-Sway test, say, is determined (*a*) by the *aptitude* of the testee, i.e., by the presence or absence of the essential ideo-motor neural bonds, and (*b*) by his *attitude*, i.e., by the degree of control or inhibition he is able and willing to exercise over the manifestations of this aptitude. Assuming that the neurotic shows less ability to control or inhibit the activity of these neuro-muscular bonds, it seemed reasonable to assume that neurotics would be more suggestible on tests of this kind than normals, a forecast shown to be correct in another paper (**16**). Similarly, it seemed reasonable to assume that the injection of such a drug as sodium amytal would not affect the responses of those who had no *aptitude* at all, because although it would weaken their control there was in their case no necessity for such control; that it would increase to a large extent, however, the reactions of those who did show a certain amount of aptitude, by decreasing their control over the manifestations of this aptitude. This forecast also was shown to be correct (**17**).

A third forecast might be made in terms of this theory, viz., that if the essential factor in such a test as the Body-Sway test is ideo-motor action, it does not matter in what way the idea is implanted in the mind of the *S;* however the implantation may take place, motor action will be found to follow. This view was tested in the experiment described in the previous section; it was found that when the *S* was simply asked to imagine that he was falling, his reaction was for all practical purposes identical with that resulting from having the suggestion that he was falling repeated to him for several minutes. In other words, suggestion was not at all necessary in order to procure the desired effects; a simple order ("Imagine that you are falling forward") had precisely the same effect.

A further point relates to the question of whether further types of suggestibility exist in addition to ideo-motor suggestibility and suggestion through indirection. It is possible that prestige suggestibility, of the kind investigated by Sorokin & Boldyreff (**27**), Arnett (**2**), Bowden (**7**), Kulp (**20**), Moore (**22**), Barry (**4**), Marple (**21**), Wheeler and Jordan (**28**), and Ferguson (**18**), may constitute a

tertiary kind of suggestibility, and an experimental clarification of this hypothesis would seem eminently desirable.

A last point which may be worthy of mention is that the amnesia which appeared in many cases after the hypnosis was not suggested, but appeared spontaneously. In fact, the patients never knew that they were being hypnotized; they were told that the purpose of the procedure was to teach them how to relax. This point is worth mentioning because many writers assume that amnesia occurs only when specifically suggested, or when the *S* knows that he is expected to forget what happened in the hypnotic state (**19**, p. 131). To the best of our knowledge, no such expectation existed in our patients, so that we would conclude tentatively that amnesia is a natural accompaniment of hypnosis.

Summary and Conclusions.—Sixty neurotic male army patients at Mill Hill Emergency Hospital, all with IQ's between 90 and 110, were given 10 tests of suggestibility, and an attempt was made to hypnotize them, and to make them carry out post-hypnotic suggestions. Also investigated in two subsidiary experiments were (1) the correlation between autosuggestion and hetero-suggestion, and (2) the effect of the length of time during which suggestion was given on the success of the suggestion. A factorial analysis was carried out on the intercorrelations between the suggestibility tests and the hypnotic and post-hypnotic scores. The following conclusions were arrived at:

(1) There are two independent types of 'suggestibility,' which may be called 'primary suggestibility' and 'secondary suggestibility.'

(2) Primary suggestibility is of the *ideo-motor* kind, and correlates highly with hypnotizability. The best single test of this type of suggestibility is the Body-Sway test.

(3) Secondary suggestibility is of the *indirection* kind, and does not correlate with hypnotizability. It can best be measured by the Odour Suggestion and the Ink-Blot Suggestion tests.

(4) A weighted point scale was constructed for the trait 'hypnotizability,' which showed this trait to be distributed continuously, and not dichotomously.

(5) This trait of hypnotizability could be forecast with considerable accuracy from knowledge of a person's scores on two or more of the suggestibility tests; a multiple correlation of .96 indicated that the tests of suggestibility involved account for 92 percent of the factors active in hypnotizability.

(6) It was shown that post-hypnotic amnesia is a natural consequence of hypnosis, and is not dependent on suggestion, either direct or indirect, to that effect.

(7) The distribution of the raw scores on tests of primary suggestibility (and to some extent on tests of secondary suggestibility also) was usually of the U-shaped type found also by earlier investigators. By application of two psycho-physical laws it was possible to convert these distributions into more nearly normal ones, and to show that the U-shaped distribution of raw scores was due to statistical and experimental artefacts.

(8) Heterosuggestion on the Body-Sway test was shown to be so highly correlated with autosuggestion that the two could not be differentiated experimentally.

(9) It was shown that the length of time during which suggestion was continued affected profoundly the effect of the suggestion.

(10) Results of the Press and Release tests of primary suggestibility suggested a subdivision of this type of suggestibility into 'active' and 'passive.' This finding was only suggestive, however.

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A REPETITION OF THE BLODGETT EXPERIMENT ON 'LATENT LEARNING'

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Introduction

The intimate connection between the latent learning experiments [1] and the non-reinforcement learning theory of Professor Tolman can be stated no more clearly than Tolman himself has done in his presidential address to the American Psychological Association.

> "During latent learning the rat is building up a 'condition' in himself, which I have designated as a set of hypotheses, and this condition—these hypotheses—do not then and there show in his behavior. S's are presented, but the corresponding R's do not function. It is only later, after a goal has been introduced which results in a strong appetite, that the R's, or as I would prefer to say, the B's, appropriate to these built-up hypotheses appear. So long as there is no appetite for what is found at the end of the maze, strong demands, plus strong hypotheses do not add up at all.[2] A strong hypothesis and a strong demand do not compensate for a weak appetite. And a strong demand and a strong appetite cannot in their turn overcome a weak hypothesis. And so on" (5).

This statement quoted above is more than an indication of the relation of a theory to certain experimental data. It is by way of being an exposition of the theory itself. This becomes clearer with an examination of the main features of Tolman's theory.

Tolman's technique of theory construction entails: (*a*) the use of a set of constructs which he terms intervening variables, reducible to a set of observables; (*b*) the relation of these constructs by a loosely formulated theory; (*c*) the identification of a second set of observables, the properties of which are held to be such as to permit confirmation

[1] It is possible to distinguish two types of latent learning experiments. The Blodgett type (1) involves giving a group of animals a series of non-rewarded runs in a maze, followed by a series of rewarded trials. The decrease in errors between the first and second rewarded runs is compared with the greatest trial to trial decrease in errors for a group of animals fed from the first trial. Differences in error-decreases in favor of the first mentioned group of animals, it has been claimed, offer evidence of a learning latent during the non-rewarded trials. The Haney (2) type involves letting a group of animals wander in a maze for several hours before the beginning of rewarded training trials. The performance of this group is compared with that of a control group running in the same maze. This control group has had no experience in the experimental maze but has been let wander in a different maze. Fewer errors throughout the course of the training trials on the part of the latent learning animals has been held to demonstrate latent learning.

[2] It is not correct to say that no 'adding-up' occurs in the non-reward period of the commonly reported latent learning experiment. Characteristically, decreases in blind alley entrances are observed (3, 7, 8).

of testable sentences drawn from the theory (**4**, **5**, **6**). For our purposes here, we need consider only a few of the empirical and intervening variables Tolman employs.

First, there are goals (*G*). These may be food, water, receptive females, etc. Second, there is the maintenance schedule (*M*), the amount of food or water or sex deprivation or the like. Third, there is the number of times the animal has been in a situation, behaved in a certain fashion and experienced the consequences of so behaving. More simply, it is the number of trials (*N*).

To these observables Tolman reduces three constructs. To the maintenance schedule, demands (*D*); to the goals, appetites (*A*); to the number of trials, hypotheses (*H*).

The second set of empirical variables embraces the behaviors of the animal, the right goings and the left goings—in short, the performance (*B*) of the rat in the maze. The theory may be stated generally

$$B = f(D, A, H).$$

According to this formulation, the law of effect is not a law of learning but of performance. Reward is not relevant for the acquisition of hypotheses but only for the use to which they are put. Hypotheses develop in the absence of reward, and once reward is introduced, they are immediately and fully employed in a manner appropriate to getting the animal to where reward is, in an economical fashion. This, Tolman claims, occurs in the latent learning experiment.

I

Of the score of latent learning experiments reported, the outstanding study has been that of Blodgett (**1**). The reasons for the unique position of the Blodgett experiment are more than historical. Blodgett's is not only the first investigation of latent learning, but the results of his study are by far the most dramatic of any yet reported.

Blodgett employed, in his experiment, a six-unit T maze, the design of which is reproduced below (Fig. 1). Three groups of animals were run in this maze. The first, a control group, was fed from the beginning of the trials. A second group was given two non-rewarded runs and was fed on the third and all following runs. A third group was given six non-rewarded runs and was fed on the seventh and all succeeding runs. All groups were given one trial per day. During the reward period the animals were allowed to eat for three min. in the goalbox, and were fed immediately the balance of their daily ration. In the non-reward period, the animals were confined in the goalbox for two min. Following this they were

removed to a cage and confined further for one hour. They were then fed their daily ration and returned to the home cage. Thirty-six animals were employed in the control and seven-day latent learning groups, and 25 in the three-day latent learning group. The error curves for these three groups are presented below (Fig. 2). It

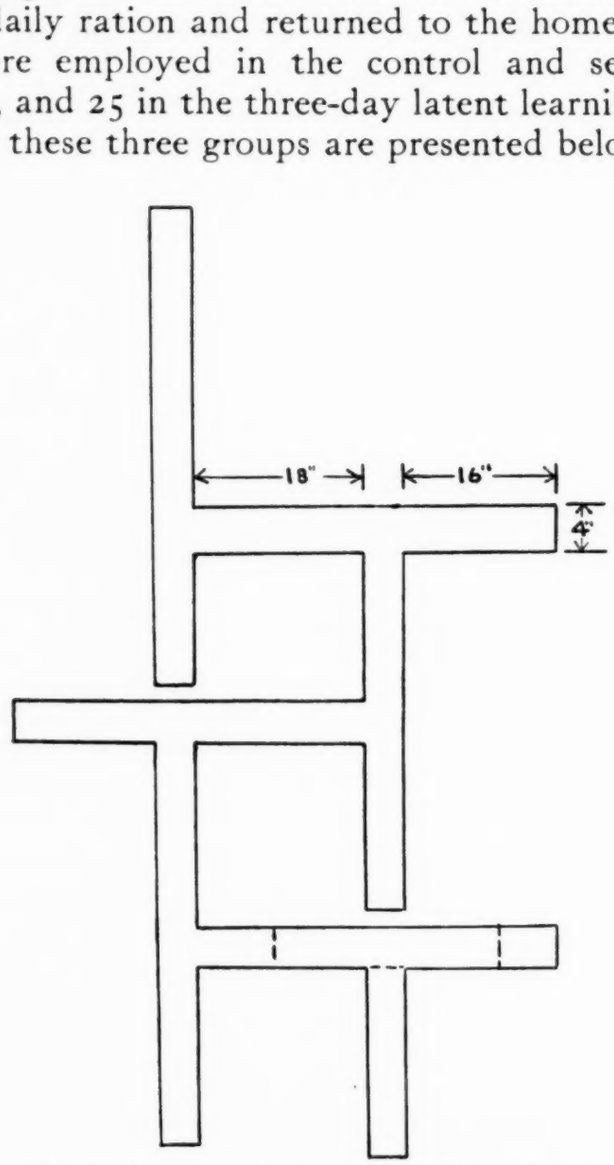

FIG. 1. Floor-plan of the Blodgett maze as constructed for use in the present experiments

can be seen that the drops in the error curves for the two latent learning groups between the first and second rewarded trials are in both cases greater than that for any trial to trial drop for the control group.

The experiment to be reported here was a repetition of the Blodgett investigation.

PROCEDURE

The procedure in the present investigation was identical, in every detail save where noted, with that employed by Blodgett. The maze was constructed with four-in. walls. The original Blodgett maze had eight-in. walls. Curtains were placed in both arms of each unit, nine in. from the choice-point, as measured from the nearest wall of the connecting alley. The maze was placed on a table some 30 in. above the floor and surrounded for a height of 36 in. by a black

curtain. The non-retrace doors were of the guillotine type rather than the hinged type used by Blodgett. The maze was of unpainted white pine, free of knots. It was lighted overhead by two 250 watt bulbs.

Blodgett fed his animals $\frac{1}{10}$ their body weight of a ration saturated with water. They were given no water other than that contained in this wet mash. In the present experiment the animals were fed six grams of Purina dog chow. This food had constituted their sole diet since weaning. One gram of this daily ration, in the case of rewarded animals, was fed in the goalbox, in the form of 'puppy pellets.' The consumption of these pellets required approximately three min. Water was available in the living cages at all times.

In the present study there were two control groups, one three-day latent learning group, and two seven-day latent learning groups.[3] One control and one seven-day latent learning group will be treated below in Experiment I. The other control group, the three-day latent learning group, and the second seven-day latent learning group will be treated in Experiment II.

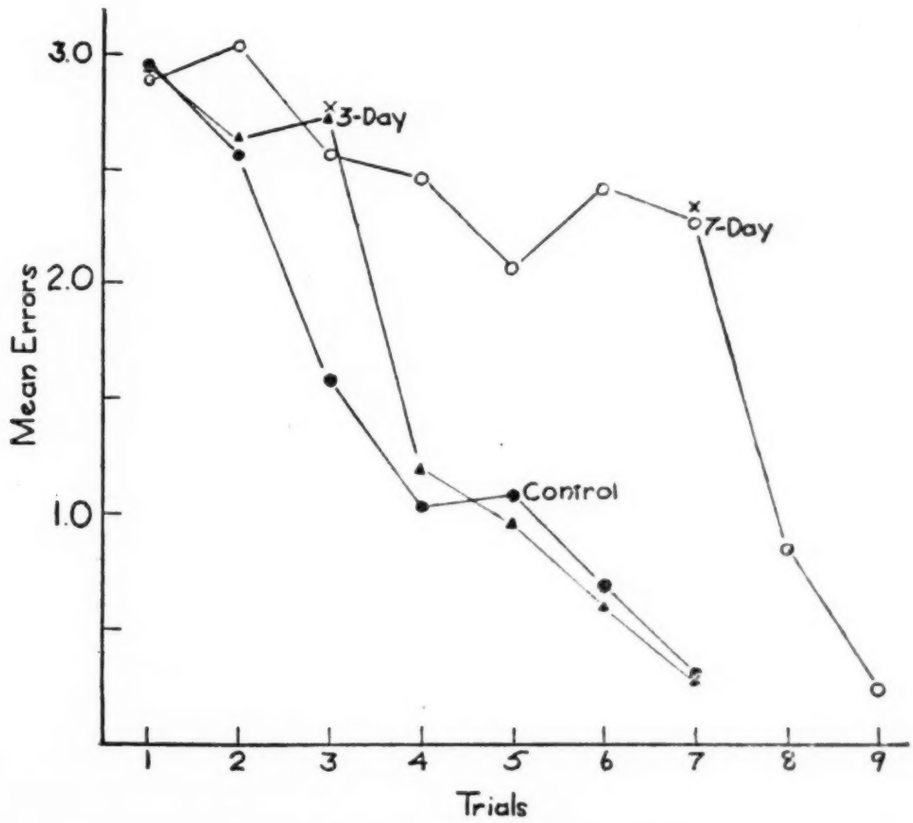

FIG. 2. Mean errors per trial for control and three-day and seven-day latent learning animals from the Blodgett experiment

All animals in Experiment I were maze-wise male albinos, none less than 150 days old. They had been employed in a spatial discrimination experiment using a simple T-maze, painted black. The groups were matched on the basis of trials to learn the discrimination problem. Each group consisted of 21 animals.

All animals in Experiment II were naïve. The control group contained 14 albino males, 90 days old. The seven-day latent learning group was composed of 20 albino males, 120 days old. There were 32 animals, 90 days old, in the three-day latent learning group. Thirteen of these were hooded females and 19 albino males. The animals were run between midnight and 4 A.M., each animal at approximately the same time each night.

The learning criteria used by Blodgett were employed in the present study. An error was recorded for any penetration of a blind to the extent that the whole of the animal's body, exclusive

[3] All animals were from the rat colony of the Department of Psychology of the State University of Iowa.

of the tail, was in the blind. Repeated entrances into the same blind on any given run were counted as a single error.

In addition to the recording of errors according to Blodgett's criteria, in Experiment II record was made of the *depth* of penetration of the blind observed in each error. If an animal entered a blind to a depth sufficient to be termed an error but turned before going through the curtain in the blind, this error was designated a part error. If the animal entered a blind and went through, or thrust his nose under or around the curtain, the error was designated a *complete* error.

Results

Experiment I.—Mean number of errors per trial for the two groups in this experiment are presented below in Table I. These same results are presented graphically in Fig. 3. The results for Blodgett's

TABLE I

Mean Number of Errors per Trial for Control and Seven-day Latent Learning Animals in Experiment I

Trials	Mean Errors									
	1	2	3	4	5	6	7	8	9	
Control . .	3.29	2.95	1.86	1.29	.91	.67	.48	—	—	$N=21$
7-day	2.81	3.29	2.95	3.0	2.76	2.24	1.57	1.14	.43	$N=21$

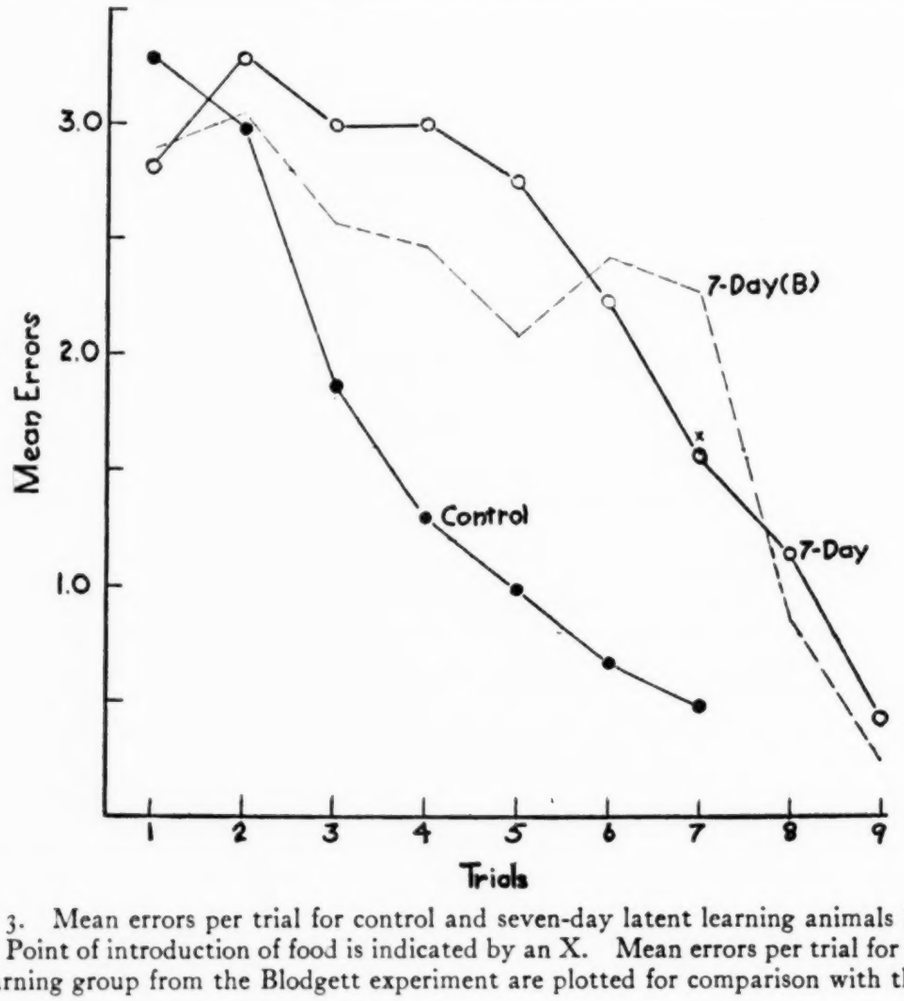

Fig. 3. Mean errors per trial for control and seven-day latent learning animals in Experiment I. Point of introduction of food is indicated by an X. Mean errors per trial for seven-day latent learning group from the Blodgett experiment are plotted for comparison with the present results.

seven-day latent learning group have been plotted on this graph for comparison with the present results.

Experiment II.—Mean numbers of errors per trial for all groups in this experiment are tabulated below (Table II). It was noted

TABLE II

MEAN NUMBER OF TOTAL AND COMPLETE ERRORS AND PERCENT PART ERRORS FOR CONTROL AND THREE-DAY AND SEVEN-DAY LATENT LEARNING ANIMALS IN EXPERIMENT II

	Trials	Errors									
		1	2	3	4	5	6	7	8	9	
Total Errors.....	Control	3.57	3.35	2.43	2.07	1.21	.57	.57	—	—	$N=14$
	3-day	3.25	3.34	3.03	2.75	2.31	1.97	1.28	.84	.59	$N=32$
	7-day	2.80	3.1	2.85	2.85	2.85	2.15	1.40	1.30	.45	$N=20$
Complete Errors.....	Control	2.29	3.14	2.07	1.00	.86	.36	.14	—	—	
	3-Day	2.19	3.03	2.41	2.13	1.44	1.16	.82	.31	.09	
	7-Day	1.9	2.75	2.05	1.50	1.15	.55	.30	.30	0.0	
Percent Part Errors..	Control	36	6.4	14.7	51.7	29.4	37.5	75.0	—	—	
	3-Day	32.7	9.3	20.6	22.7	38.7	41.3	36.6	62.9	84.4	
	7-Day	32.1	11.3	28.1	47.4	59.6	74.4	78.6	76.9	100	

above, that data on depth of penetration were recorded. These data have been treated as follows: for each trial the number of *part* errors (non-under-the-curtain errors) have been divided by the number of total errors to give percent part errors of total errors; for each trial mean number of *complete* errors (under-the-curtain errors) have been calculated. These have been tabulated (Table II) for both latent learning groups.

Mean number of errors per trial are presented graphically in Fig. 4. Blodgett's results for his seven-day latent learning and three-day latent learning groups are plotted for purposes of comparison. In Fig. 5, percent *part* errors for each trial have been plotted for both latent learning groups. Mean number of *complete* errors for each trial for both latent learning groups have been plotted and are presented in Fig. 6. Mean errors per trial have been plotted on this last graph for comparison.

Mean running times per trial for all groups are presented in Table III. The results observed in the case of running time in the maze are consistent with results observed in the elimination of blinds in the present experiments. A discussion of the significance of running time of animals in the latent learning experiment will be presented in a later paper.

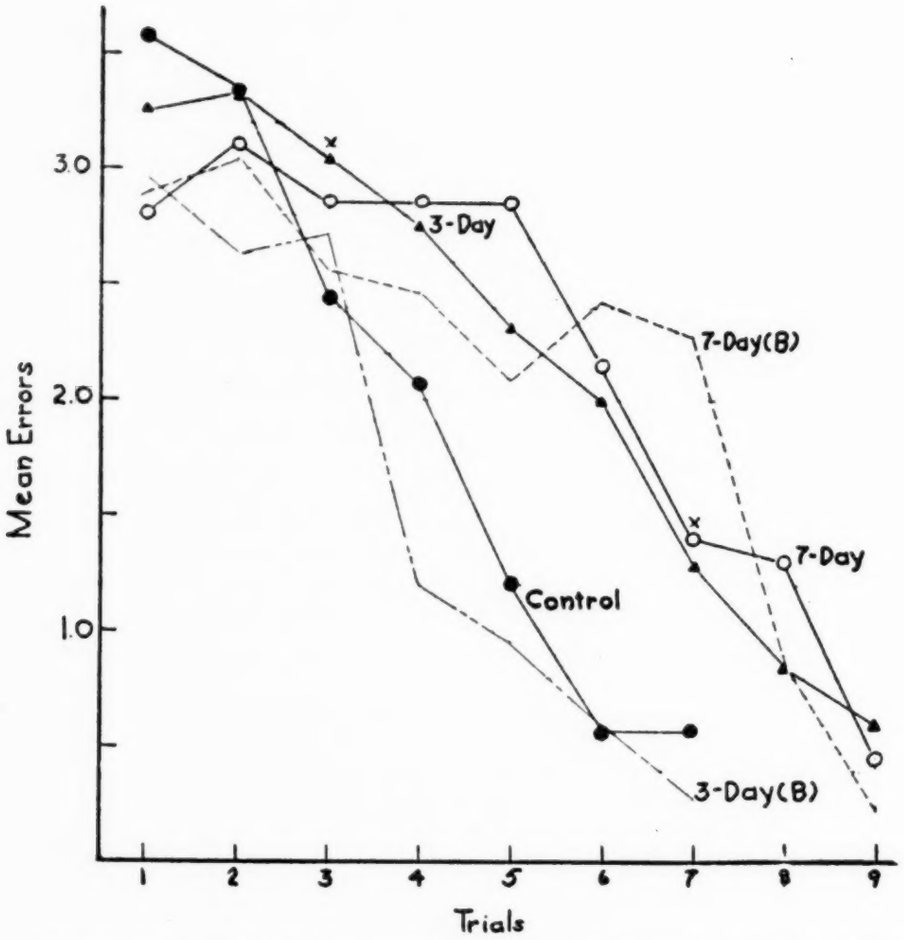

FIG. 4. Mean errors per trial for control and three-day and seven-day latent learning animals in Experiment II. Point of introduction of food is indicated by an X. Mean errors per trial for three-day and seven-day latent learning groups from the Blodgett experiment are plotted for comparison with the present results.

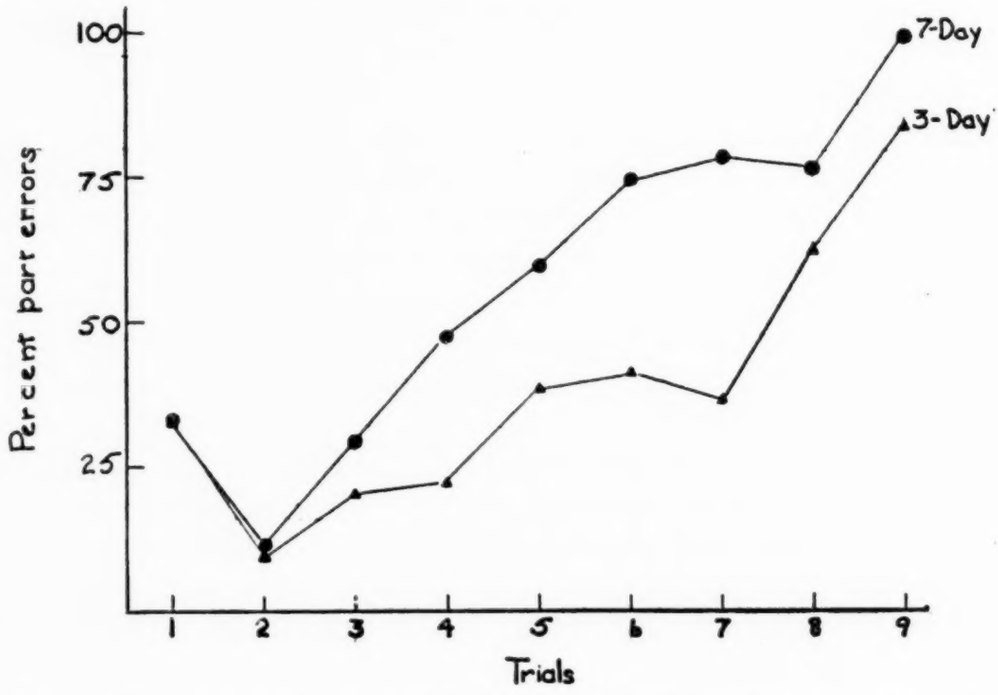

FIG. 5. Percent part errors per trial (Part errors/Total errors) for three-day and seven-day latent learning animals in Experiment II

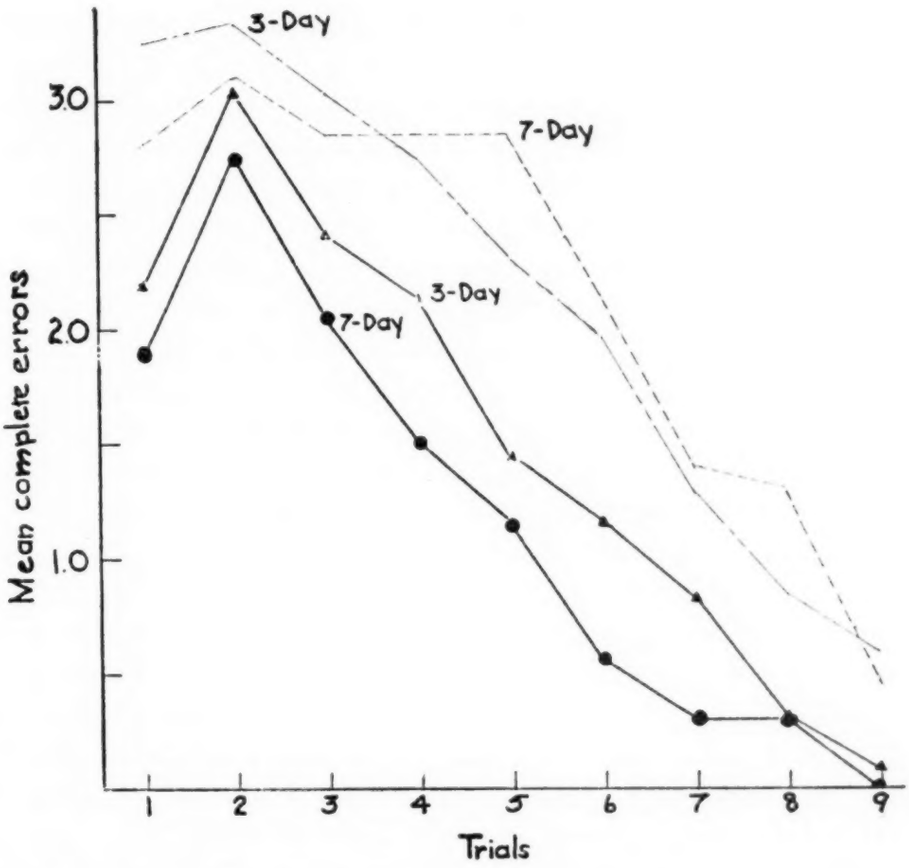

FIG. 6. Mean complete errors per trial for three-day and seven-day latent learning animals in Experiment II. Mean errors are plotted to permit comparison with mean complete errors.

DISCUSSION OF RESULTS

Experiment I.—When the results of the seven-day latent learning and control groups of the present experiment are compared with those obtained by Blodgett, it can be seen that both similarities and differences between the two sets of data exist. The error curve for the control group in the present study is higher on the Y axis than that for the control group in Blodgett's study. There is an essential identity in the general shape of the two curves.

TABLE III

MEAN RUNNING TIME PER TRIAL FOR ALL GROUPS IN EXPERIMENTS I AND II, WITH DIFFERENCES BETWEEN CRITICAL TRIALS

Trial	1	2	3	4	5	6	7	8	9	Diff. Critical Trials
Control Exp. I.....	81.0	57.5	30.1	25.0	20.2	18.7	16.0	—	—	23.5
7-Day Exp. I.....	92.0	79.5	72.8	78.5	100.1	52.6	35.1	21.5	13.9	13.6
Control Exp. II.....	119.2	74.9	50.1	32.8	24.4	26.8	21.3	—	—	44.3
7-Day Exp. II.....	172.8	111.6	90.6	92.5	81.6	91.5	60.0	24.5	11.1	35.5
3-Day Exp. II.....	139.9	81.2	75.9	58.3	32.5	25.8	21.0	16.1	14.0	17.6

The error curve for the latent learning group in the present experiment is above that for the same group in the Blodgett study, in the early trials. The two curves show some similarity in shape. Both curves rise at the second trial. In the later trials, however, a very striking and significant difference is manifested between the two curves. At the sixth and seventh trials the error curve for the animals from the present study *continues to fall* in a manner consistent with the course of fall up to these trials. The curve for the Blodgett animals rises at the sixth and seventh trials in a manner inconsistent with the course of fall up to these trials. The significance of this divergence of the present results from those reported by Blodgett is apparent when the drop in the error curves for the two latent learning groups between the seventh and eighth trials is compared with the greatest trial to trial drop of the two control groups. In the Blodgett study the control group curve shows its greatest fall between the second and third trials—.98. In the present study the drop between the same two trials for the control group is 1.09. The drop between the seventh and eighth trials in the curve for the latent learning group in the Blodgett experiment is 1.42. The difference between decreases in errors for the control and latent learning groups is .548, in favor of the latent learning group. In the present study the drop between the seventh and eighth trials for the latent learning group is .43. The difference in decreases in errors for the control and latent learning groups between the indicated trials is .63, in favor of the control group. *In brief, in the present experiment no latent learning was observed.* What was observed was that the latent learning group *learned during the non-reward period.*

Experiment II.—When we compare the error curve for the control group in Experiment II with those of the controls from Experiment I and the Blodgett study, we observe that it is considerably higher on the Y axis than either of these latter two. The general shape of the curve is similar to that of the other control group curves. Again we note a retardation at the second trial. The decrease in mean errors between the second and third trials is .92.

The error curve for the seven-day latent learning group in Experiment II is above that for the same group in the Blodgett experiment. It is below that for the same group in Experiment I, during the early non-reward trials. The general shape of the curve is similar to that observed in the other two curves. There is the characteristic rise at the second trial. Like the error curve for the seven-day latent learning group in Experiment I, and unlike the error curve for the same group in the Blodgett study, the error curve for the seven-day latent learning group in Experiment II continues to *fall* during the sixth and seventh trials.

The decrease in mean errors from the seventh to the eighth trial for the present latent learning group is only .10. This compares with the .92 decrease for the control group noted above. This gives a difference in favor of the control group of .82.

In the case of the seven-day latent learning group in Experiment II, *latent learning as manifested by a sudden drop in the error curve upon the introduction of reward was not observed.* Learning was evidenced throughout the course of the trials, whether the trials were reinforced or not.

We turn now to a consideration of the three-day latent learning group in Experiment II. The general shape of the curve in the early trials is similar to that observed in both latent learning groups in the present study, considered above, and to the error curve for the seven-day latent learning group in the Blodgett experiment. It shows the characteristic rise at the second trial noted in the other three error curves, and the following drop to approximately the same level as observed at the first trial. Following the introduction of food on the third run, the error curve falls from 3.03 to 2.75, a decrease of .28. This approximates in magnitude the .22 decrease between the first and second rewarded trials of the control. It is considerably less than the greatest trial to trial decrease for the control group. The difference between the decrease in errors following introduction of reward for the three-day latent learning and the largest trial to trial decrease of the control group is .64.

When we compare the results of the three-day latent learning group with the same group in the Blodgett study, we note that not only is there no agreement between the results of the two groups following the introduction of reward, but there is little agreement before reward. It was noted above that the error curve for the three-day latent learning group, in the present study, rises at the second trial and falls at the third trial. This is consistent with the characteristics of the error curves for all seven-day latent learning groups discussed above. The error curve for the three-day latent learning group in the Blodgett experiment, however, *falls* at the second trial and *rises* at the third trial.

In concluding this discussion of the present results two facts should be noted. First, in the three latent learning groups in Experiments I and II, no evidence of any latent learning was observed. Second, in the case of the seven-day groups in both experiments, learning in the pre-reward period occurred.

II

We turn now to a consideration of the data from the present study on depth of penetration of blinds. It will be remembered that errors

recorded in Experiment II were differentiated on the basis of whether or not, in making an error, the animal went under the curtain in the blind. If the animal turned short of the curtain the error was designated a *part* error. If the animal went under, or nosed under or around the curtain, the error was designated a *complete* error. Since gradual elimination of blinds characterizes the learning of animals in mazes, it was anticipated that recording of part and complete errors in the present experiment would offer opportunity to observe such indication of learning in the pre-reward period. That such gradual elimination of blinds occurred can be seen at once by considering Figs. 5 and 6. Unfortunately our measures of depth of penetration were so gross as to obscure differences between part errors. It can be said at this point, however, that the preponderant number of part errors made in the later non-rewarded runs of the seven-day latent learning group involved penetrations of the blinds barely sufficient to determine an error.

Two measures of depth of penetration are offered here. We shall consider these in turn for the seven-day and three-day groups. The first of these is percent part errors of total errors. Examination of Table II and Fig. 5 shows that percent part errors increased from 11.3 on the second trial to 78.5 on the seventh run, for the seven-day group. This is an increase of 600 percent.

The second measure is mean number of complete errors. This is 2.75 for the second and .30 for the seventh run for the seven-day group. This is a difference of 2.75, with a sigma of .27.

Turning to the data on depth of penetration of blinds for the three-day latent learning group, we note that percent part errors at the third trial was 20.6. Mean number of complete errors was 2.41. This was accompanied by a mean number of total errors of 3.03 at this trial. Percent part errors for the fourth trial was 22.7, a decrease of only 2.1. Mean number of complete errors was 2.13 on the fourth trial, a decrease of .28. Mean number of total errors on the same trial was 2.75. The amount of learning observed with the introduction of food can be seen to be neither marked for total errors nor for depth of penetration of blinds.

Implications of Gradual Elimination of Blinds for the General Problem of Latent Learning

The results of the present study relating to gradual elimination of blinds have implications which extend beyond this investigation and that of Blodgett. If it is characteristic of animals, in the non-reward period of latent learning experiments, to reduce the depth of penetration of the blinds, then drops in learning curves with the introduction of food are hardly occasion for surprise. Differences

between decreases in errors for latent learning animals and control animals, under these circumstances, can hardly be claimed to evidence latent learning. Control animals show their greatest drop in errors early in the trials. At this point most errors involve, in the main, complete entrances into the blinds. For the control group in Experiment II, percent part errors on the trial prior to the greatest decrease in errors was 6.4. Latent learning animals show greatest decreases in errors late in the trials, when most errors involve incomplete entrances into the blinds. It is obvious that these latter error decreases are quite different from the error decreases of the control animals. The elimination of complete errors cannot be compared with the elimination of part errors. This is particularly true when such part errors entail the animal's entering a blind a distance but little greater in extent than the length of his body. Greater decreases in errors for latent learning animals than for control animals can be taken as indication of latent learning only on the assumption that the errors are equivalent in both cases. The data considered above make such an assumption tenuous in the extreme.

Summary

1. The Blodgett latent learning study was repeated with two groups of animals.

2. One group of animals contained maze-wise male albino rats. Twenty-one animals were fed from the beginning of training and 21 from the seventh trial.

3. The other group contained only naïve animals. Fourteen albino males were fed from the first trial; 20 albino males were fed from the seventh trial; and 19 albino males and 13 hooded females were fed from the third trial.

4. It was observed that the seven-day animals learned during the non-reward period. Both seven-day and three-day groups showed a decrease in depth of penetration of blinds before the introduction of food.

5. No instance of latent learning was observed in any of the delayed reward animals in either main group.

6. The data on decrease of penetration of blinds were held to offer explanation of such latent learning as has been observed in studies of the Blodgett type.

(Manuscript received April 25, 1945)

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THE EFFECTS OF ATTITUDES ON DESCRIPTIONS OF PICTURES

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This paper deals with the effects of three hypnotically induced attitudes—happiness, criticalness, and anxiety—upon observation and thought. Three *S*s described a series of pictures when under the influence of each of these attitudes.

Introduction

Laymen as well as psychologists are well aware that attitudes, sets, moods, and other internal conditions may greatly affect both perception and thought. Nevertheless, it is only in recent years that a few experimental psychologists have brought the intellectual effects of these internal conditions under scientific scrutiny. When perception, memory, and intelligence are studied in the laboratory under simplified, objective, unemotional—almost inhuman—circumstances, factors like anxiety or resentment, which are of the highest importance in the everyday functioning of these processes, are overlooked. Gardner Murphy brings this out forcefully in his 1944 presidential address to the American Psychological Association (**6**).

A new trend is indicated by Bartlett's (**1**) and Rapaport's (**9**) studies of the effects of attitudes and emotions on remembering, by Murray's (**7**) study of the influence of fear upon children's estimates of the maliciousness of other personalities, and by Sanford's (**10, 11**) and Levine, Chein, and Murphy's (**5**) investigations of the perceptual distortions produced by various degrees of hunger.

Though projective techniques were originally developed to discover an individual's attitudes and other characteristics, they can also be useful for discovering the influence of known attitudes on perception and thought. Proshansky (**8**) in a recent experiment found that liberals and conservatives, as determined by an attitude scale, could be reliably detected from their descriptions of suitably chosen pictures. In an unpublished experiment we found that, according to three judges, there were three times as many indications of anti-labor sentiments in the descriptions of six photographs of economic scenes by two anti-labor *S*s as in those by two pro-labor *S*s.

In these studies, the experimenters limited themselves to investigating the effects of already present attitudes on the description of pictures. Under ordinary circumstances the creation in the laboratory of the particular attitudes one wishes to study is difficult if not almost impossible. By using hypnosis, however, almost any attitude, set, or mood can be very readily created. In hypnosis the experimental psychologist has a technique for controlling variables in situations otherwise beyond the reach of psychological experimentation (3). Erickson's investigations provide many illustrations of the successful use of post-hypnotic suggestions for the creation and study of such moods as resentment or jealousy (2). Other investigators have been able to study the effects of hypnotically induced elation, depression, and hypochondria upon Rorschach records (4).

Procedure

The present experiment involved the description of six pictures by three *S*s when in each of three different moods: happy, critical, and anxious. The *S*s—upperclass students in their early twenties, one man and two girls—were picked from a number of volunteers because of the ease with which they could be made to pass into the deepest stages of hypnosis.

The six photographs were chosen from current magazines. They showed young people of college age in a variety of situations, such as attending a seminar, digging in a swampy area, jitterbugging, engaging in battle activities, and lying on a sunny campus.

Each of the three *S*s completed his portion of the experiment at one sitting and independently of the others. Each knew nothing of the experiment's purpose. The process of creating each of the three moods was done gradually, after the *S* had been deeply hypnotized. The final suggestion for the happy mood was as follows: "Now you are feeling very happy and you are in a cheerful and joyous mood. You feel as if everything is rosy and you are very optimistic. You have a comfortable feeling of well-being; nothing is worrying you. You feel perfectly at peace with everything and everyone. You are in a very happy, cheerful, and optimistic mood." It was noticed incidentally that facial expression tended to change with each mood.

After the *S* had described all six pictures, he was told to relax, close his eyes, and rest awhile. The happy mood was removed and the *S* was brought back to his 'normal' hypnotic state. He was told that he would forget having seen the pictures just shown and that he would also forget what he had said about them. Then the *S* was gradually put into a critical mood. The final suggestion was: "Now you are very critical; you are quick to find fault and to condemn unfavorably. Your judgment of others is very harsh and severe. You see failings and faults very clearly. You are very critical and fault finding." The procedure of showing the six pictures, taking down the descriptions, then wiping out the induced mood, and suggesting amnesia for the pictures and for what had been said, was the same as for the happy mood.

The final suggestion in producing the last or anxious mood was: "Now you are quite anxious. You are disturbed over some possible misfortunes. You are disquieted and concerned as to something in the future. You are a little fearful and mildly alarmed. You have a feeling as if you were expecting something disagreeable to happen, yet were not sure that it would. You are quite anxious."

In each mood, Subject A was told to tell something about what was seen, as well as exactly what he saw in each picture. As a result A's reports tend toward accounts of what he thought about the situation pictured, or of what it reminded him. Subjects B and C were told to tell only what they saw in each picture; consequently they limited themselves more to what was actually in the picture and added less interpretative material. The pictures had no titles and were shown one by one always in the same order. None of the *S*s showed any recollection of having seen the pictures while in the preceding mood. The *E* wrote down each *S*'s description of each picture as he gave it (see tables on the following pages).

TABLE I

HAPPY ATTITUDE

Pictures	Subject A	Subject B	Subject C
I. Young people digging in a swampy area.	It looks like fun; reminds me of summer. That's what life is for: working out in the open, really living—digging in the dirt, planting, watching things growing.	Nice summer day. Working. Cool feet in water. Nice lot of food when they get through.	Boys and girls digging. Trees, stream, shovel. Mud all over them. Corn or wheat and shadow of a mountain in the background. Digging for irrigation or maybe planting something.
II. Colored people jitterbugging.	Having a good time. No matter what people say, they have a sense of rhythm. We whites miss that enjoyment; we worry too much.	Village—some night when they're really hot and having a good time. Just hilariousness.	Men and women jitterbugging. Zoot suit. Satin shorts on girl. They're having a good time; maybe a play of some sort.
III. Wounded man being carried on litter by soldiers to aeroplane.	Wounded soldier. Good thing men were there to help him and get him to a hospital. The men in this war are well taken care of.	Restfulness after a hard day's fight. Taking a guy back to safety from a battlefield.	Man being carried by litter bearers. Clouds and palms, canteen and flying jackets. Wounded being taken to a transport plane. Maybe Florida.
IV. Seminar around fireplace at faculty home.	Wonderful faculty group—it's wonderful, you don't find it everywhere. That's what's important in college; less book-learning.	Fireplace, home. All relaxed. Just a feeling of mellowness.	Woman reading to a group of people. Fireplace, clock, figures on mantle, books, fire. I like this. Very comfortable looking. Looks like fireplace at home.
V. Waist gunner in action in a bomber.	It's funny how they dress them up—protected from injury by machine. Now, they care more about the individual. Best possible working conditions.	Lot of work and exertion. Firing a gun in the heat of battle.	Looks like a monkey. Machine gun, empty cartridges. Oxygen mask, heavy clothing. Excellent composition, good photography.
VI. Four college men on sunny lawn typing, reading, listening to radio.	Before the war. Boys lying on campus listening to radio and reading. Not getting much done but having a good time. That's fun though.	Complete relaxation. Not much to do—just sit, listen, and relax. Not much at all to think about.	Sunbathers, radio, books, studying and typing in the sunshine. It must be a very warm and restful place.

TABLE II

CRITICAL ATTITUDE

Pictures	Subject A	Subject B	Subject C
I. Young people digging in a swampy area.	Pretty horrible land. There ought to be something more useful for kids of that age to do instead of digging in that stuff. It's filthy and dirty and good for nothing.	Digging a ditch in slimy mud. Girls working there too, tsk, tsk. Looks like pretty hard work.	People digging and getting all dirty. The spade is not being held right. Don't look very clean. Stalks all battered down.
II. Colored people jitterbugging.	Negroes jitterbugging as usual; that's all they do. Unaesthetic; no one enjoys looking at it. Nothing good about it. They should be directed into other than useless activity.	One guy looks deformed. Seems to be having a good time though. Kid looks pretty young. Savoy or New York City. Probably all lit.	Bunch of crazy jitterbugs. He looks like a runt. Sort of silly because of the clothes and the movements they're making. All night binge. Don't like checks with stripes. Only one looks half decent.
III. Wounded man being carried on litter by soldiers to aeroplane.	Wounded or killed soldier; one more in a million who are just killing each other off. That's war, I guess. We must think it's fun or we wouldn't do something so useless as murder and destruction.	Wounded man being taken to safety, but a lot of good it does now. Looks as if they're in a hurry—must be pretty badly hurt. Must have been a tough scrap.	Fellows carrying away sleeping man. Movie set. Hands don't look natural or as if they're carrying something. The picture is artificial and was posed for.
IV. Seminar around fireplace at faculty home.	People sitting around talking. Sometimes we run up blind alleys arguing for the sake of argument.	Guy here looks disgusted. Grandmother looks bored. One gal is looking daggers. Grandpa doesn't look comfortable.	Woman reading to a bunch of unhappy people. Everything in the room is too formal. Reader is not comfortable. Girl with middy looks droopy. White-haired woman has a long face. I don't like picture.
V. Waist gunner in action in a bomber.	Typical example of man hiding away from himself. Closing himself in a factory. He doesn't know where he is going. Same thing every day and for what!	Having a hard time. Has been fighting steady for quite a while. Looks mean with goggles and maybe the guy he is shooting at looks just as mean.	Oh God! Man shooting gun. Why don't they pick up the shells? Messy. No room to move around.
VI. Four college men on sunny lawn, typing, reading, listening to radio.	Typical college campus. Everybody loafing. Typing a letter he doesn't need to type. Slacking on their work.	Someone ruining a good pair of pressed pants by lying down like that. They're unsuccessfully trying to study.	Somebody trying to get a tan, wasting time. Not comfortable. Tie coming undone, sole of shoe worn out. Messy. Leaves unraked.

TABLE III

ANXIOUS ATTITUDE

Pictures	Subject A	Subject B	Subject C
I. Young people digging in a swampy area.	They're going to get hurt or cut. There should be someone older there who knows what to do in case of accident. I wonder how deep the water is.	Digging in a hurry to get rid of flood waters. Must be pretty important to get it done.	Burying something or digging something. Wide open field and mountain in the distance.
II. Colored people jitterbugging.	Look as though they're having a good time; it seems as though they shouldn't be because something may happen to their families.	Good time. I wonder how they'll feel tomorrow. Probably back to driving trucks.	People jitterbugging. I hope we don't look that way when we jitterbug. I don't like the picture.
III. Wounded man being carried on litter by soldiers to aeroplane.	He is wounded and they're taking him to a plane but he's in bad shape and may not live even though the plane will rush him to a hospital.	Everybody in a hurry to get him back to safety. Must be emergency case, more so than usual. Man trying to act not too far gone but that doesn't mean he isn't.	Wounded man being carried off by buddies. Fliers, lots of boys like that. The man on the litter has a sweet face. I hate to have something like this happen; it frightens me.
IV. Seminar around fireplace at faculty home.	Mr. G. and some students. The picture is not right because Mr. G. isn't living anymore. You never know when tragedy is going to hit.	Solving a big family problem. Everybody looking on. Anxious look on Grandmother and Sister. Thoughtful look on Grandpop's face.	Lot of people discussing. Lady reading to them. Somber atmosphere. It makes me feel as if I don't want to go to anyone's house.
V. Waist gunner in action in a bomber.	I wonder if it is safe—seems like the floor is badly cluttered. If anything happened I wonder if anyone would know. He might become tangled up in the rigging and be helpless.	Having a hard battle. Wonder if he'll win it. I bet he wonders what is going to happen to him.	Like monster from Mars. He's in a battle and perhaps he'll be killed. Don't like it because it reminds me of the war. Depressing.
VI. Four college men on sunny lawn typing, reading, listening to radio.	Boys relaxing on college campus. By now they may be overseas or wounded or dead. They never know whether they'll come through alive.	They're listening to a football game or world series. Probably a tight game. One guy looks as if his side wasn't winning.	Lot of men. Peaceful enough. I wonder if they want to come back, and what's going on in their minds. Are they fighting now?

Results and Conclusions

The induced attitudes are influential in determining what the *S* observes. In the happy mood, A describes picture IV as 'a wonderful faculty group'; but in the anxious mood A's attention is focused entirely upon one recently deceased member of that group. The moods are much more noticeable, however, in their effects on the interpretation of what is observed. The meanings and feelings attached to the activities shown in the pictures and the probable causes and results of those activities are usually very different from mood to mood. In a happy frame of mind the *S*s see the soldier in picture III 'as being well taken care of,' and as being taken 'back to safety,' or to a 'transport plane.' When in an anxious mood these same *S*s say the soldier 'is in bad shape,' 'may not live,' 'an emergency case,' 'it frightens me.' After viewing picture VI, when in a happy mood, A says the boys are 'lying on campus . . . having a good time.' But when in a critical mood, A describes the boys as 'loafing'; and when in an anxious mood, A thinks of them as maybe 'overseas, or wounded, or dead.' Attitudes exert a directive influence both on what is observed and on the train of thoughts suggested by the observations.

To gain a more objective appraisal of the extent to which the three attitudes actually influenced observation and thought, the 54 statements shown in the charts were typed on slips of paper and, after being thoroughly shuffled, were submitted to three judges. The instructions given the judges were: "The statements you have been given are brief descriptions of pictures. Indicate how many times, if any, each of these three attitudes or moods are expressed or implied in each statement: (1) Happiness—cheerfulness, joy, optimism, well-being, satisfaction, approval of things as they are, contentment; (2) Criticalness—fault-finding, condemnation, disapproval, dissatisfaction with things as they are; (3) Anxiety—concern over the outcome of events, foreseeing unfavorable outcomes."

Descriptions made by the *S*s when in a happy mood averaged, according to the three judges, 2.33 indications of happiness, .24 indications of criticalness and .13 indications of anxiety (Table IV). Under the critical mood, the average statement had three indications of criticalness and negligible indications of each of the other two attitudes. The effect of the anxious mood was not quite so clearcut: anxiety was indicated 1.44 times per statement, happiness .17 times, and criticalness .50 times. In every case the difference between the mean number of indications of the induced mood and the mean number of indications of each of the other two moods was statistically significant.

TABLE IV

EFFECTS OF THREE MOODS (HAPPY, CRITICAL, AND ANXIOUS) ON THE DESCRIPTIONS OF SIX PICTURES BY THREE SUBJECTS

Mood	Happy			Critical			Anxious		
Mean number of times each mood was indicated per picture	Happy	Critical	Anxious	Happy	Critical	Anxious	Happy	Critical	Anxious
	2.33 σ = .96	.24 σ = .39	.13 σ = .17	.09 σ = .30	3.15 σ = 1.41	.11 σ = .23	.17 σ = .22	.50 σ = .57	1.44 σ = .78
Statistical reliability of the differences between the means	$M_h - M_c = 2.09$ S.E.$_{dif.}$ = .24; *C.R.* = 8.7			$M_c - M_h = 3.06$ S.E.$_{dif.}$ = .35; *C.R.* = 8.7			$M_a - M_h = 1.27$ S.E.$_{dif.}$ = .21; *C.R.* = 6.1		
	$M_h - M_a = 2.20$ S.E.$_{dif.}$ = .23; *C.R.* = 9.5			$M_c - M_a = 3.04$ S.E.$_{dif.}$ = .35; *C.R.* = 8.6			$M_a - M_c = .94$ S.E.$_{dif.}$ = .25; *C.R.* = 3.8		

An anxious mood, as developed in this experiment, apparently tends to produce an appreciable number of remarks which three judges label as indicators of criticalness. The judges sometimes found it difficult to decide whether the remarks made during the anxious mood were critical, anxious, or neutral. The great majority of the descriptions written in the happy or critical moods, however, did not contain even a single remark which, according to the judges, was indicative of a mood other than the induced one. In some instances the mood exercised such a profound influence on perception that the picture would not be recognized from the description as the same one described in the other two moods.

These results would seem to indicate that (1) common sense and clinical insight are correct in assigning a major role to moods, feelings, and attitudes in the determination of intellectual processes; and that (2) even very brief descriptions of suitably chosen pictures show clearly the effects of a dominant attitude. Further experiments might be designed to discover what incidents typical of everyday life can create such powerful, directive attitudes, how these attitudes exercise their effects on perception and thought, and how these effects might be controlled. Hypnosis should prove an invaluable tool in creating the conditions necessary for such experiments.

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ERRATA

Attention is called to the following errors in the article by M. E. Bitterman, Heart rate and frequency of blinking as indices of visual efficiency, THIS JOURNAL, 1945, **35**, 279–292:

Page 286, last line above Table II, read "lower" for "higher."

Page 288, Fig. 4. The key fails to match the curves. The highest curve was obtained with 91 foot-candles; to match the key, the curve should be drawn with a solid line. The lowest curve was obtained with 3 foot-candles; the key for 3 foot-candles should be a solid line between open circles.